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THE ROYAL SOCIETY OF EDINBURGH

PROCEEDINGS

Volume 10

NOVEMBER 1878—JULY 1880

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OF

THE ROYAL SOCIETY

OF

EDINBURGH.

VOL. X.

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ERRATA.

Page 700, line 8, *for* "Microphone," *read* "Telephone."

„ 702, line 14, *for* "onset," *read* "set."

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. X. | 1878–79. | No. 103.

Ninety-Sixth Session.

Monday, 26th November 1878.

Sir WILLIAM THOMSON in the Chair.

The following Council were elected:—

President.
Professor KELLAND, M.A.

Honorary Vice-Presidents.
His Grace the DUKE of ARGYLL.
Sir ROBERT CHRISTISON, Bart., M.D.
Sir WILLIAM THOMSON, Knt., LL.D.

Vice-Presidents.
David Stevenson, Memb. Inst. C.E.
The Right Rev. Bishop Cotterill.
Principal Sir Alex. Grant, Bart.
David Milne Home, LL.D.
Sir C. Wyville Thomson, LL.D.
Prof. Douglas Maclagan, M.D.

General Secretary—Dr John Hutton Balfour.

Secretaries to Ordinary Meetings.
Professor Tait.
Professor Turner.

Treasurer—David Smith, Esq.

Curator of Library and Museum—Alexander Buchan, M.A.

Councillors.
Professor Fleeming Jenkin.
Rev. R. Boog Watson.
Dr Hugh Cleghorn.
Professor T. R. Fraser.
Professor Rutherford.
Dr R. M. Ferguson.
Rev. W. Lindsay Alexander, D.D.
Dr Thomas A. G. Balfour.
J. Y. Buchanan, M.A.
Rev. Thomas Brown.
Robert Gray.
Dr William Robertson.

Monday, 2d December 1878.

Professor Kelland, the President, read the following Introductory Address:—

In taking my place this evening, I might reasonably be expected to say much about my unworthiness to fill the post, and the kindness of my friends in placing me here. All that I can trust myself to say is, that I feel too deeply everything that can be imagined of this kind to venture on giving it words. To be the successor of such men as the Duke of Argyll, Sir Robert Christison, and Sir Wm. Thomson, is an honour which the most ambitious man might covet, and the most self-conceited deem himself scarce worthy of. To myself that honour has come neither to gratify ambition nor to administer to self-conceit. It has descended on me all unsought through the kindness of the many friends who have sat with me for years in this room, and the only emotion it awakens is that of affection and gratitude. Just a month has elapsed since it became apparent to me that I should be called upon to address you to-night. That such would be the case had not till then crossed my thoughts. I had made no preparation for the address. The first month of the University session left me the very smallest amount of time and strength for the work. You will, therefore, pardon an address rather feebler in character than is fitted to the occasion. Happily, the kindness of friends has aided me very materially in the preparation of the obituary notices. Mr Milne Home has placed at my disposal documents, both in print and in MS., relative to Sir Richard Griffith—the latter, unfortunately, arrived late on Saturday evening, when I had completed my brief sketch of Sir Richard, but I hope it may be allowed me to avail myself of these documents in preparing the sketch for the press. Sir Robert Christison has kindly furnished remembrances of Hugh Scott of Gala and Sir James Coxe, of which I have availed myself; and Mr Gordon, through Professor Balfour, has furnished me with sundry published obituary notices. I have to add that the notice of Fries is entirely due to Professor Balfour, that of Regnault to Professors Tait and Crum Brown, that of Claude Bernard to Professor Rutherford, that of Mr Cunningham to Professor Duns, and that of Harkness to Professor Geikie. The notice of Martyn Roberts is due to his family.

The following statement in regard to the number of the present Fellows of the Society has been drawn up by the Secretary :—

1. Honorary Fellows—

Royal Personage—

His Royal Highness the Prince of Wales, 1

British Subjects—

John Couch Adams, Esq., Cambridge; Sir George Biddell Airy, Greenwich; Thomas Andrews, M.D., Belfast (Queen's College); Thomas Carlyle, Esq., London; Arthur Cayley, Esq., Cambridge; Charles Darwin, Esq., Down, Bromley, Kent; John Anthony Froude, Esq., London; Thomas Henry Huxley, LL.D., London; James Prescott Joule, LL.D., Cliffpoint, Higher Broughton, Manchester; William Lassell, Esq., Maidenhead; Rev. Dr Humphrey Lloyd, Dublin; William Hallowes Miller, LL.D., Cambridge; Richard Owen, Esq., London; Thomas Romney Robinson, D.D., Armagh; General Sir Edward Sabine, R.A., London; Henry John Stephen Smith, Esq., Oxford; Professor Balfour Stewart, Manchester; George Gabriel Stokes, Esq., Cambridge; James Joseph Sylvester, LL.D., Baltimore; Alfred Tennyson, Esq., Freshwater, Isle of Wight, . . 20

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Foveign—

Robert Wilhelm Bunsen, Heidelberg; Michel Eugène Chevreul, Paris; James D. Dana, LL.D., Newhaven, Connecticut; Alphonse de Candolle, Geneva; Heinrich Wilhelm Dove, Berlin; Jean Baptiste Dumas, Paris; Charles Dupin, Paris; Professor Carl Gegenbaur, Heidelberg; Herman Helmholtz, Berlin; August Kekulé, Bonn; Gustav Robert Kirchoff, Heidelberg; Herman Kolbe, Leipzig; Albert Kölliker, Würzburg; Ernst Eduard Kummer, Berlin; Johann von Lamont, Munich; Richard Lepsius, Berlin; Ferdinand de Lesseps, Paris; Rudolph Leuckart, Leipzig; Joseph Liouville, Paris; Carl Ludwig, Leipzig; Professor J. N. Madvig, Copenhagen; Henri Milne-Edwards, Paris; Theodor Mommsen, Berlin; Louis Pasteur, Paris; Professor Benjamin Peirce, U.S. Survey; Karl Theodor von

Carry forward, 21

Brought forward, 21

Siebold, Munich; Otto Struve, Pulkowa, St Petersburg; Bernard Studer, Berne; Otto Torell, Lund; Rudolph Virchow, Berlin; Wilhelm Eduard Weber, Göttingen; Friedrich Wöhler, Göttingen, . . . 32

Total number of Honorary Fellows at November 1878, ... 53

The following are the Honorary Fellows deceased during the year :—

Foreign—Victor Regnault, Claude Bernard, Elias Magnus Fries, Angelo Secchi, ... 4

British—Sir Richard Griffith, ... 1

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2. Ordinary Fellows—

Ordinary Fellows at November 1877, ... 373

New Fellows, 1877–78.—W. H. Allchin, M.R.C.P.; Dr Andrew Peebles Aitken; John Frederick Bateman, Esq.; Charles Davidson Bell, Esq.; James Blyth, Esq.; James Brunlees, Esq.; Dr John Archibald Campbell; Dr Daniel John Cunningham; John Grahame Dalziel, Esq.; Dr Samuel Drew; Dr J. J. Kirk Duncanson; James Alfred Ewing, Esq.; R. K. Galloway, Esq.; Lord Inverurie; William King, Esq.; P. R. Scott Lang, Esq.; Alan Macdougall, Esq.; Dr Alexander Macfarlane; John Milne, Esq.; George M'Gowan, Esq.; Dr Richard Norris; James R. Stewart, Esq.; Robert Macfie Thorburn, Esq.; Rev. John Wilson, ... 24

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Deduct Deceased.—Hugh Scott, Esq. of Gala; Sir William Stirling Maxwell, Bart.; Sir William Gibson-Craig, Bart.; Sir James Coxe; Professor Robert Harkness; Dr James Watson; James Cunningham, Esq., W.S.; Martyn J. Roberts, Esq.; Dr Edward James Shearman; Dr James Allan, who died in 1852, but whose death was only intimated in 1878, ... 10

Resigned.—Dr Thomas Smith Maccall; Dr Thomas E. Thorpe, ... 2

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Total number of Ordinary Fellows at November 1878, 385

Add Honorary Fellows, ... 53

Total Ordinary and Honorary Fellows at commencement of Session 1878–79, ... 438

During the last Session, the Neill Prize for the triennial period 1874–77 was awarded to Dr Traquair for his paper on the "Structure and Affinities of *Tristichopterus alatus*" (Egerton), published in the twenty-seventh volume of our "Transactions;" and also for the many contributions he has made to the knowledge of the structure of recent and fossil fishes.

Among our Foreign Honorary Fellows, Henri Victor Regnault deservedly held a foremost place, especially as an experimental philosopher, alike in chemistry and in physics. Born at Aix-la-Chapelle, July 21st 1811, he came to Paris in his early youth with the sole object of obtaining a livelihood. While engaged as a shopman in a bazaar, he made such good use of his scanty leisure as to qualify himself for admission to the *École Polytechnique* (1830). In 1832 he became a pupil in the *École des Mines.* Thereafter he was for sometime a Professor in Lyons; but, in 1840, returned to Paris, having been elected a Member of the Academy of Sciences in consequence of important investigations in Organic Chemistry. He became, in 1847, *Ingénieur-en-chef* of the second class, and Professor of Chemistry in the *École Polytechnique,* and of Physics in the *Collège de France,* and was made Director of the Imperial Porcelain Manufactory at Sèvres in 1854.

Regnault's first published chemical work was on the action of potash on the oil of the Dutch chemists (1835). His discovery of the bodies now known as chloride, bromide, and iodide of vinyl had a very important bearing on the development of the radical theory, and his speculations on the relations of these substances to aldehyde brought about a temporary agreement in opinion as to the constitution of acetic acid and analogous substances between Berzelius, Liebig, and Dumas.

In 1838 and 1839, Regnault published investigations on the action of chlorine on the oil of Dutch chemists and on hydrochloric ether. He stated, in a very precise form, a view as to the persistency of molecular arrangement, which (along with the views already expressed by Dumas and by Laurent) contributed to found the "Substitution Theory," over which Berzelius and the French chemists had a long and bitter controversy. The two investigations

just referred to led to the discovery of cases of isomerism of great theoretical importance.

Regnault's investigations of specific heat of metals reduced the number of apparent exceptions to the law of Dulong and Petit, and induced him to propose (in 1840–41, and again in 1849) the change in the atomic weights of silver and of the alkali metals, which was afterwards strongly advocated by Cannizaro, and is now generally adopted.

In physical work Regnault was distinguished rather for extreme skill in manipulation and patient study of details (especially in the investigation of necessary corrections) than for brilliance or novelty in discovery. He was an admirable experimenter, and may be said to have done almost as much good to science by training a school of skilled experimenters as by his own extended researches. He devoted himself specially to the accurate determination of physical constants, such as latent and specific heats, to the laws of expansion of gases and vapours, to the determination of the densities of gases, and specially to the accurate measurement of temperature.

His greatest work, forming Volume XXI. of the *Mémoires de l'Académie des Sciences*, was undertaken for the French Government, and contains most elaborate experimental determinations of the various physical data required for investigations of the working of steam engines. Besides numerous other chemical and physical papers (a complete list of which will be found in the Royal Society's catalogue of scientific papers), Regnault published, in four volumes, an "Elementary Course of Chemistry," which has been long and deservedly successful.

The death, during the siege of Paris, of his only son, who was rapidly advancing to high distinction as a painter, seems to have clouded his later years, and he died on January 19, 1878, at the age of 67.

Claude Bernard.—By the recent death of Claude Bernard, France has lost her greatest physiologist, and this Society one of the most distinguished of its Foreign Associates.

Bernard was born in 1814, at St Julien in France; he studied medicine in Paris, became assistant to Magendie, resolved to devote

his life to the pursuit of physiology, and at the age of 40 he was appointed to a chair of physiology, specially created for him by the Faculty of Sciences of Paris.

The services which he rendered to physiology and medicine are so eminently distinguished, that his name must be ranked with those of Harvey and Haller, of Bichat and Müller, of Magendie and Charles Bell. Bichat and Magendie were his countrymen, and it was by the brilliant teaching and example of Magendie that he was inspired, and induced to offer his genius and his labour to the cause of physiological science.

The great influence of Magendie's teaching over the young mind of Bernard was due to the circumstance, that as a teacher of an experimental science he did not content himself with the delivery of mere didactic discourses, but sought, as far as lay in his power, to experimentally demonstrate the truth of what he stated, and the several steps by which that truth had been ascertained. The truths of physiological science thus received a living power which no mere words could give, and the evolution of Bernard as a physiologist was one of its results.

One of his great discoveries had reference to the liver, an organ whose function—although it is imperfectly comprehended even now—was greatly elucidated by his researches. Previous to his time, it was supposed that the secretion of bile was the only function of this organ; Bernard, however, made the remarkable discovery that the liver also produces glycogen—a starch-like substance which is converted into grape-sugar as it passes from the liver into the blood. This discovery greatly advanced our conceptions of the nature of the chemical processes that take place in the animal organism, for it was previously supposed that starch and sugar are produced by the tissues of plants only.

With regard to the sugar-forming function of the liver, Bernard also discovered that by injuring a certain part of the medulla oblongata of a rabbit, the sugar-forming function of the liver is greatly exaggerated. The excess of sugar poured into the blood is excreted by the kidneys, and thus the disease known as diabetes is artificially induced. Probably no discovery ever produced a more startling effect on the medical world; for this simple experiment on a *rabbit* afforded the first rational explanation of a disease that is

far from being rare in the human subject, and which had completely baffled the skill of physicians to furnish any reasonable theory as to its causes.

Another of his great discoveries had reference to the influence of the nervous system over the bloodvessels. He showed that the bloodvessels are influenced by two sets of nerves—one causing diminution of their calibre by exciting their muscular fibres to contraction, the other giving rise to dilation of the vessels.

This discovery offered for the first time the true explanation of changes in the calibre of the bloodvessels occurring in the various bodily organs during their states of rest and activity; and which are commonly observed in the face in the conditions of blushing and pallor. The whole question of the innervation of the bloodvessels is one of great difficulty, which in many of its details even now baffles the investigator; yet although Bernard did not at first fully grasp the significance of some of his experiments, he nevertheless gave the key to all the subsequent observations; and the accuracy of not one of his experiments has ever been gainsaid. His *observations* were exact, although his interpretations were not in every case entirely correct.

Another of his great investigations had reference to the function of the pancreas. By an elaborate research, he proved that the fluid which this important organ pours into the alimentary canal powerfully affects the fatty elements of the food, converting them into an emulsion, and partly saponifying them, so that they may be readily absorbed by the lacteals.

Another important research had reference to the effects of *carbonic oxide* and *curara.* He showed that carbonic oxide produces suffocation by combining with the blood-pigment, and thus rendering that substance unable to discharge its normal function of conveying oxygen from the lungs to the tissues.

With regard to the Indian arrow poison—curara, he proved that it produces a paralysis of motion by acting on the terminations of the motor nerves in the muscles. The method of physiological analysis by which he proved this is a model for all researches of a similar nature.

The importance of Bernard's researches on these poisonous substances lay in the circumstance that they were conducted from a

physiological point of view; and they may be said to have been the first to convince the leaders of medical thought, that a true knowledge of the actions of poisonous and medicinal agents can only be arrived at by a thorough investigation of their effects on the animal organism in a *state of health*, combined with observations of their effects in *diseased conditions.* A wide stream of research already flows from this conviction, and practical medicine is constantly deriving increasing benefit therefrom.

In addition to these—his greatest works—Bernard also made important observations on the functions of the fifth, seventh, and eighth cranial nerves; on recurrent sensibility; on the secretion of the salivary, gastric, and intestinal juices; on the temperature of the blood in the right and left sides of the heart; on the gases of the blood and their variations as the blood circulates through organs in a state of rest as compared with a state of activity; on the modifications in the secretions of the stomach and intestinal canal after removal of the kidneys; and on the production of albuminuria by lesions of the nervous system.

Bernard was adored by his pupils, not only because of his greatness as an investigator and as a teacher, but also on account of the enthusiasm with which he inspired them, and the unceasing spirit of affection and encouragement which he ever manifested towards them. Happily for physiology and for medicine, he gave no countenance to that sentiment which would deter from performing a painful experiment on an animal, for the purpose of eventually saving pain and saving life both of man and of animals; while it is silent with regard to the vast amount of suffering inflicted on animals for purposes that are frivolous and unnecessary.

Those who knew Bernard best can aver that he was a man of kindly disposition, who lived a blameless life, and devoted himself faithfully and only too earnestly to the advancement of medical science,—almost to the very day of his death, on the 10th day of February last.

He deserved well of his country, for he had done as much as the greatest of his predecessors, and the most renowned of his contemporaries, to keep her in the foremost rank of science; and France was not slow to recognise the greatness and the unselfish character of the service he had rendered to her, as well as to science; for her

deputies unanimously awarded to him the pomp and ceremony of a State funeral, thus magnifying his name and his example.

He was worthy of the exceptional honour which France paid to his memory, and his name and his work will last while medical science endures.

Happily for France, his mantle has worthily fallen on Brown-Séquard, Paul Bert, Vulpian, Marey, and Moreau, who have already amply proved themselves worthy of so great a master.

We have to record the death of another Honorary Fellow of the Society—Elias Magnus Fries, a distinguished Professor of Botany in the University of Upsala, in Sweden. He was born at Smaland in August 1794, and died at Upsala on the 8th of February 1878. His father was pastor at Femsjö, and was fond of botanical pursuits. Even at an early period of life young Fries accompanied his father on botanical rambles. During one of them he picked a very showy species of Hydnum, which seems to have turned his mind to the study of Agarics and other fungi.

Fries was one of the great promoters of Scandinavian science. His works embraced all departments of botany, but his attention was specially directed to Lichens and Fungi. His early studies were prosecuted at a school in Wexiö. In 1811 he became a pupil at the University of Lund, where he studied under Schwartz, Agardh, and Retzius. In 1814 he was chosen Docent of Botany. At this time he published his first work, entitled "Novitiæ Floræ Suecicæ." In 1847 he was elected a member of the Royal Academy of Sweden, and in 1851 he became Professor of Botany in the University of Upsala, vacant by the resignation of Wahlenberg. This chair he continued to occupy until within a few years of his death. He continued to publish works in the Scandinavian language, specially on Mycology and Lichenology, up to 1874. The state of Fries' health did not permit him to join the great celebration of the 400th anniversary of the University of Upsala in September 1877. He presented some of his foreign botanical friends with copies of his photograph on the occasion. His son is now Professor of Botany at Upsala. In the Royal Society's Catalogue of Scientific Papers there are enumerated eighty-five separate publications by Fries, extending from the year 1816 to the year 1874.

Angelo Secchi was born at Reggio on the 29th June 1818, and received his early education in the schools of the Jesuit Fathers. He at the outset distinguished himself in mathematics and physics, and for a time lectured on those subjects in the Collegio Romano. In 1844 he commenced his theological studies. Three years later, on the Revolution of 1847, he was obliged to take refuge in England, and was ordained priest at the College of Stoneyhurst. From thence he passed to America, and was made Professor of Physics in Georgetown College, where, however, he remained only a very short time. The death, in 1848, in London, of Father Francesco di Vico, Director of the Observatory and Professor of Astronomy in the Collegio Romano, brought Secchi back from America as his successor. Here he laboured for thirty years in accumulating and publishing observations, astronomical and meteorological, for which the Papal Government, aided by private liberality, furnished him with excellent instruments and an ample personal staff. His astronomical observations were published in three vols. 4to, extending from 1851 to 1856. So far as I know, they came down no further.

Secchi, though an excellent observer and a man of great power, was of a discursive turn of mind. He had little power of concentration, and appears early to have tired of the monotony of astronomical observations, and to have turned his attention to the more popular studies of terrestrial magnetism and solar physics. His attention to the latter subject had probably been aroused by his having assisted Professor Henry, when in America, in making the first experiments on the heat radiated by different portions of the sun's disc, by means of the thermo-electric pile. His interest in spectroscopy dates from Janssen's first visit to Rome, and he turned it to good account, having published, in 1847 and 1848, spectroscopic observations on more than three hundred stars. The same subject is treated in a volume entitled "The Stars," published first about the time of his death, which will, it is believed, prove to be a work of great importance, and likely to procure for its author a lasting reputation.

In 1871 there was formed a Society, calling itself the *Società degli Spettroscopisti Italiani*, two of the principal workers in which were Secchi at Rome and Tacchini at Palermo. Thanks to the liberal supply of funds by the Government, the two observa-

tories of Palermo and Rome were enabled to carry on a daily series of combined observations, principally on the sun. The results of these observations and of the other labours of this Society are published periodically at Palermo, and have already reached the seventh volume. Secchi photographed the eclipse of 1860 in Spain, and observed that of 1870 in Sicily.

In 1862 Secchi commenced a monthly series in 4to, entitled the *Bulletino Meteorologico*, consisting of daily observations, meteorologic and magnetic, made both at the college and at various places in the neighbourhood of Rome, as well as of observations made on the sun's spots. This collection, now edited by Father Ferrari, has reached its sixteenth volume.

The grand *Exposition Universelle* of 1867 procured a favourable opportunity of exhibiting his registering meteorograph in Paris, for which he obtained the great French prize of 100,000 francs (?) and the Grand Cross of the Legion of Honour, which the Emperor Napoleon conferred on him with his own hand. He took the opportunity whilst in Paris of delivering some lectures, a portion of which have been published in French, in 2 vols. 4to, under the title of *Le Soleil.*

When the Collegio Romano passed from the Papal to the Italian Government, the Chair of Astronomy in the new Roman University was offered to Secchi and accepted, but the chief of his order would not allow him to retain it. His connection with the Observatory did not, however, cease.

Secchi's reputation was undoubtedly very great and wide-spread. He was member of a very large number of scientific societies. Amongst the rest, the Royal Society of London elected him one of their foreign members in 1856, and our Society followed their example in 1865. His great merit consisted in industry and activity—his error, in want of definiteness of aim, in over-production. The Royal Society's Catalogue of Scientific Papers contains a list, carried down to 1863 only, of no less than 230 contributions to scientific journals. At the time of his death, which took place on the 26th of February last, this list must have been greatly extended. Their value is probably not in proportion to their extent; but it cannot be doubted that they contain much that will help on the future progress of science.

Mr Hugh Scott of Gala was born in 1822. He held a commission as captain in the 92d Highlanders, and was afterwards major in the Dumfries, Roxburgh, and Selkirkshire Militia. He was a Justice of the Peace and Deputy-Lieutenant of the county of Selkirk, and an enthusiastic supporter of the Episcopal Church of Scotland, whose cause he advocated for years in the local and Edinburgh newspapers. His descent, both by his father's side and that of his mother (who was daughter of Sir Archibald Hope of Craighall), secured him a good position amongst the landed gentry of Scotland, and his personal qualities were of the highest order. Spite of a marked stutter, he shone in society, and was always a general favourite; indeed, it may be doubted whether this defect of speech is not an aid rather than a hindrance to its possessor, whether as a converser or as a lecturer. Charles Kingsley was a notable example amongst those who have passed away; and many members of this Society will call to mind living examples illustrative of the truth of this remark.

Mr Scott died at Hyères, whither he had gone in search of health, on the 19th of December last.

James Cunningham, Esq., W.S., was born at Edinburgh on the 18th of March 1800. He died on the 6th of November 1878. His father, Alexander Cunningham, W.S., was a lineal descendant of Alexander Cunningham the historian, younger son of the Rev. James Cunningham, who was ordained minister of Ettrick in 1641. Alexander Cunningham's grandfather married a sister of Dr Robertson, Principal of the University of Edinburgh, and grand-uncle of the late Lord Brougham. In Chambers's edition of Burns, Alexander Cunningham is referred to as the chief Edinburgh friend of the poet. Mr Cunningham was educated at the High School of Edinburgh, in Mr Gray's class, along with the late Lord Neaves, Professor Syme, Dr James Begbie, and other afterwards well-known citizens. After serving an apprenticeship in the office of Messrs Gibson-Craigs & Wardlaw, W.S., he was admitted a member of the Society of Writers to Her Majesty's Signet in 1823, and shortly after he began practice as a Writer to the Signet in partnership with the late James Walker, Esq. He retired from business in 1852, and in the same year was elected a Fellow of this Society,

whose meetings he attended with much regularity, and in whose proceedings he took a great interest. The personal friend of the leading Scottish naturalists of the generation now passing away, Mr Cunningham was a most intelligent and earnest amateur student of natural science.

Mr Cunningham was twice married—first, in 1836, to Margaret Sheaffe Bagot, sister of the Rev. Daniel Bagot, Dean of Drumore; and secondly, in 1846, to Elizabeth, daughter of Alexander Dunlop of Keppoch, and sister of the late Alexander Murray Dunlop of Corsock. Mrs Cunningham, four sons, and a daughter survive.

Dr James Watson was a native of Glasgow, where he was born on the 11th of September 1792. He was educated at the High School and University of Edinburgh, where he took the degree of M.D. in 1812, before he had completed his twentieth year. Early in life he received the appointment of assistant-surgeon in the East India Company's service, and resided for nearly twenty years in India. On his return from India in 1831 he retired from the Company's service, and shortly afterwards settled in Bath, where he soon acquired a very large practice, and was unquestionably the leading physician in Bath and the adjacent counties. Amongst his other patients must be noted Prince Louis Napoleon after his escape from Ham. He was an enthusiastic member of his profession, devoting himself to hospital work with untiring zeal, and in his later years of comparative leisure bringing the experience of his lengthened career to bear on the subjects of hospital administration and hospital finance. In private life he was a pleasant and instructive companion, with a vigorous intellect, which remained unclouded to the last. He died on the 27th of September last, having just completed his eighty-sixth year.

Dr Edward James Shearman was a native of Wington, in Somersetshire. The house in which he was born was next door to that of Mrs Hannah More. Very soon after completing his medical studies he settled in Rotherham, where he practised his profession for the space of upwards of fifty years. His contributions to medical literature have been numerous and varied. The particular department to which he appears largely to have devoted his atten-

tion was the use of the microscope as a guide to the diagnosis and the prevention of disease. More than a year before the publication of the first edition of Dr Golding Bird's "Urinary Deposits," he read before the Sheffield Medico-Chirurgical Society an "Essay on the Changes in the Urine affected by Disease, and the Tests to distinguish them," which was published in the *Lancet.* He became medical officer to the Rotherham Board of Health, but his microscopic examination of the town water gave such offence to that Board that they speedily got rid of him. He had, however, done his work, and he cared little for the consequences. He had thoroughly opened the eyes of the people, and a new era followed. In his scientific tastes he was somewhat discursive, and too apt to wander from subject to subject. To this circumstance we may attribute his failure to attain that eminence amongst his contemporaries which his talents and enthusiastic devotion to his profession, and to every study connected with it, would have secured him, had he concentrated his attention on some special subject. As it was, he was a useful and thoroughly instructed medical man, loved and respected by a wide circle of patients and friends. He died in his eighty-first year.

Sir James Coxe was born at Gorgie in 1811. His father died when he was young, and the bringing up of the family devolved on his mother, the sister of George and Andrew Combe.

In early life he pursued his studies on the Continent, and acquired a competent knowledge of French and German. He graduated in the University of Edinburgh in 1835, and shortly afterwards joined the College of Physicians, on the list of whose Fellows he stood fourth at his death. From the commencement of his professional career he gave considerable attention to diseases of the mind, in which he was no doubt encouraged by his uncles the Combes. He was naturally of a shy and reserved disposition, and thus his good points, indomitable industry and strong common sense, were long hidden from the public, to his no small injury. Indeed, had it not been for his marriage in 1841, to the sister of Dr William Cumming, which resulted in his being brought under the notice of the Duke of Argyll, he might probably have waited long for the opportunity of making his powers known to the world. As it was, Sir James

was appointed in 1855 Member of a Royal Commission to inquire into the management of the insane in Scotland. The writing of the Report fell chiefly to Sir James, and it disclosed such a chaotic condition of the arrangements for the care of the insane, and such an amount of neglect and cruelty, as shocked and surprised Parliament and the country. To the ability with which this Report was drawn up may be attributed Sir James's ultimate position in his profession. The Report was presented to Parliament in 1857, and it led in the same year to the passing of Lord Moncreiff's Lunacy Act. Under this Act Sir James became one of the paid Commissioners, which appointment he held till his death, giving to the discharge of his duties the most earnest and conscientious attention.* The first fifteen of the Reports of the Commissioners were entirely written by Sir James, and they prove incontestably that he was a man pre-eminently fitted for the post, not only by his early education and his acquired tastes, but by his power of concentration, which led him to throw his whole life into the work. He visited English and Continental establishments for the care of the insane, and made himself master of their methods of treatment. He thus became strong in his efforts to improve the state of things at home. At his suggestion amendments were made in the Act of 1857, in 1862, and again in 1866, and these have given it a character so special as to attract the attention of Continental and Colonial Governments. Sir James was fortunate in having Dr Arthur Mitchell as a colleague, as the successor to Dr Browne, when he was unfortunately laid aside by blindness. Dr Mitchell not only stood with him in carrying out his views during his life, but is in a position to keep up their efficient working now that he is gone. Sir James was examined before the Parliamentary Committee on Lunacy in 1877, and his evidence was reckoned the most valuable given on that occasion.

As a literary man, he devoted his time mainly to assisting his uncles in their labours, and during the last two or three years of his life he gave much of his spare time to the superintendence of the

* He was ably supported by his co-commissioner Dr W. A. F. Browne, one of the most eminent of British psychologists, who was instrumental in introducing the modern system of treatment of the insane in the Montrose Asylum and the Crichton Royal Institution, Dumfries.

memoir of his uncle George Combe, written by Mr Charles Gibbon. Sir James received the honour of Knighthood in 1843—an honour well merited from the zeal and devotion with which he pursued a line of conduct which has resulted, spite of much opposition, in conferring great benefits on his country. The personal character and general bearing of Sir James were such as in a good cause to overcome opposition; determined but quiet, armed at every point, but cautious in the use of his weapons, himself thoroughly convinced, but never treating lightly the opposite convictions of others, he was enabled to discharge his delicate and difficult duties in a manner to bear down opposition so softly and insensibly as to leave no trace of wounds behind. He was seized with illness in Paris, whither he had gone for relaxation, and died at Folkstone on the 9th of May last.

Sir Richard Griffith, who has been styled "the Father of Irish Geology," was born in Dublin on the 20th of September 1784. He was descended from a Welsh family of distinction, his ancestors having come over to Ireland about the commencement of the seventeenth century, and acquired considerable property in various parts of the country. The bulk of the property having lapsed from the family through failure of issue, the grandfather of Sir Richard disposed of the remainder, and settled in Mellicent, county Kildare, marrying a kinswoman (Miss Elizabeth Griffith, of Glamorganshire), by whom he had issue Richard Griffith, the father of the deceased. The son, also named Richard, was educated in Dublin, with a view to obtaining a commission in the Irish Artillery, in which he succeeded in 1799 after passing the usual examination. He retained his appointment only a short time, the Act of Union having broken up the separate establishments of the two countries. The offer was, indeed, made him of an appointment in the British forces, but his father caused him to decline it, conceiving that a better opening was afforded him in the practice of civil engineering. He accordingly directed his attention to the study of mines, and at seventeen proceeded to Cornwall, with the view of gaining a practical knowledge of mining. Here his assiduity attracted the attention of Sir Humphry Davy. On Mr Griffith discovering in the Dalcoath mine the rich ores of nickel and cobalt which were raised with the silver ore,

but had till then been rejected as rubbish, Lord de Dunstanville, one of the principal proprietors of the mines, offered him a permanent appointment in them, which, however, he declined, preferring to give his studies a wider range, probably with a view to devote his powers to the service of his native country. He accordingly visited the mining districts of Derbyshire, Yorkshire, Durham, and Northumberland. This brought him to Edinburgh, where he formed the acquaintance of Sir James Hall, Professors Playfair, Jamieson, and Hope, by whom he was held in such high estimation, that in 1808, when only twenty-three years of age, he was unanimously elected a Fellow of this Society. He now returned to Dublin, and, under the influence of the Royal Dublin Society, at once commenced "a geological and mining examination of the Leinster coal district." The publication of the results of his labours in this field was completed in 1814. In 1809 he was selected by the Commission appointed to inquire into the practicability of draining and improving the bogs of Ireland to be one of their engineers. In 1812 his surveys and reports were published by the authority of Parliament. At this date he received the appointment of Inspector-General of the Royal Mines in Ireland, as successor to the eminent mineralogist Richard Kirwan. Three years later he issued the first instalment of a geological map of his native country, to which he regularly made additions during the space of forty years, when it was published in a completed form. The Geological Society of London in 1855, in recognition of its value, awarded him the Wollaston Palladium Medal. Professor Edward Forbes, in presenting the medal, described the map as "one of the most remarkable productions which had ever been effected by a single geologist." This map he had the honour of presenting personally to Her Majesty, who took a lively interest in it. The result was that in 1858 Griffith was created a Baronet.

It is a remarkable testimony to the knowledge which Sir Richard had early acquired of the geology of his native country, that in the map he had coloured a district as Upper Silurian, whilst the officers of the Geological Survey who followed him held it to belong to a formation between the Silurian and the Old Red Sandstone. Sir Richard, however, adhered to his opinion. To get the matter settled, Mr Hall, the present director of the Geological Survey of

Ireland, went during last autumn with some of his staff to the district, and became satisfied of the correctness of Sir Richard's view. He at once communicated the fact to Sir Richard in a letter, which, however, reached him too late to give him the satisfaction which it was intended and calculated to afford.

The famine in the south of Ireland, which occurred in 1822, aroused the then Lord-Lieutenant, the Marquis of Wellesley, to energetic action in the improvement of the means of communication between different parts of the country. He accordingly appointed Griffith as engineer, to improve and construct roads in the counties of Cork, Kerry, and Limerick. Under his direction the starving population were employed to construct some 250 miles of road through districts hitherto accessible only with great difficulty, and not a little danger from the disaffected population, whom it was not easy to render amenable to British authority. While engaged in this work he received the important appointment of General Boundary Surveyor, the magnitude of whose duties may be inferred from the fact that 1000 miles of the boundaries of about 69,000 Crown lands had to be ascertained and settled. In 1827 he was appointed Commissioner of Valuation under Mr Goulburn's Act, and "Griffith's Valuation" was accepted as the test of value in nearly all the land disputes in Ireland. In 1846, after the great potato famine, he was appointed Deputy-Chairman of the Board of Works, the onerous duties of which appointment he performed regardless of self, working thirteen hours a day without food or drink, feeling that the lives of thousands depended on his exertions. It will not be necessary to allude to the various appointments which he held during his long life, nor to the numerous public works which were executed under his superintendence. That which is best known in his native city was the diversion of the course of the Liffey, and the conversion of a group of dilapidated houses, the nursery of vice and fever, into the esplanade which stands in front of the Royal Barracks.

Sir Richard Griffith was seventy-four years of age when he was created a Baronet, an age at which most men relax from their labours or cease them altogether. It was very different with Sir Richard. He had still twenty years of good work left in him. The preservation of his physical and mental powers for so long a period

he attributed to his temperate habits of living, being an early riser and remarkably abstemious. At eighty, when on a geological excursion with his friend Mr Milne Home, he once walked eighteen miles in a day. He continued to give and receive hospitalities as late as to his ninety-second year. In private life he was upright and honourable, kindly and sociable. He was married in 1812 to Miss Waldie of Hendersyde Park, Kelso, with whom he celebrated a golden wedding in 1862. His long and useful life was brought to a close on the 22d September 1878.

Mr Milne Home, who knew him well, has kindly communicated to me some of Sir Richard's letters to members of his family. They breathe a tone of simple and unaffected piety, which gilds the other virtues of this excellent man.

In the death of Sir WILLIAM STIRLING MAXWELL, Scotland has lost (to use the words of Lord Houghton) her first man of letters. Sir William's name is familiar to literary men on both sides of the Tweed, not as the Baronet, but as simply William Stirling of Keir. With that name he commenced his career, with that name he obtained his earliest and brightest laurels, and with that name he will descend to posterity.

He was born at Kenmure, near Glasgow, on the 8th March 1818. On his father's side he was descended from the Stirlings of Keir, retainers of the house of Stuart, and famous in history. On his mother's side he traced his pedigree up to the battle of Otterburn, where had been shed the blood of a Maxwell of Pollock. His after career was tinged with his ancestral associations, which operated to throw his mind with affection back on the past.

Mr Stirling completed his early education by taking the degree of B.A. in Cambridge in 1839. He was Fellow Commoner of Trinity College during my residence in the University, but, so far as I know, did not distinguish himself as a student. He devoted himself rather to art and history than to those early studies which constitute the framework of the training of a University. His love for travel was early developed, and the ample means at his disposal enabled him to indulge in it in a manner and to an extent not in the power of ordinary students. It was during his residence at Cambridge as an undergraduate that he made the tour in Palestine,

which brought out his first appearance in print in a small volume, entitled "The Songs of the Holy Land." Like most young men, Stirling's first essays in writing were in verse, and like most wise men, his efforts in verse ceased with the first effusion. His good genius left him free ever after to express himself in prose. After leaving Cambridge he lived much on the Continent; and having a facility for the acquisition of languages, he became a tolerable proficient in French, Italian, and Spanish. It was the last-named language, and the literature which it opened up, that seized on his youthful mind and influenced the whole current of his future thoughts. Spain was at that time little visited by the British traveller, and the literature of the country was to the ordinary student sealed up in an unknown tongue. Prescott and Ford, and others with them, have since given us an insight into the treasures, artistic and literary, which are stored up in the Peninsula. To Stirling they came with all the freshness of original discovery.

The early history of the Moor and the Cid, tinctured with romance, and having its roots in the struggles of the mind after religion, was peculiarly attractive to a young man whose natural bent was to the sombre in art and the ascetic in religion. He returned again and again to Spain, and familiarised himself with her literature and her art—so unlike the literature and art of northern Europe. To the latter branch of study he at first devoted his attention. Spanish art, unlike Italian, is characterised by its positive features of religion and decorum, and is no less marked by its negative features of deficiency in landscape, in marine, and in animal painting. The Church, the supreme power in Spain, discouraged the study of anatomy; and the result is, that the subjects in which the Italians most delighted were shunned or neglected by the Spaniards. The consequence was, that to those whose education was based on the Italian school, the Spanish treatment of sacred subjects seemed dry and unintelligible. Thus, whilst Raphael and Michael Angelo were familiar to the minds of our countrymen (as they deserved to be), the not much inferior, but very different, greatness of Morales, of Zurbaran, and of Velazquez was but coldly recognised. Few had seen them in the Madrid gallery, and the few who had seen even Velazquez, the greatest of them, had not yet begun to recognise him as the artist who "drew the minds of men."

Wilkie, writing half a century ago, described the Peninsula as "an unexplored territory—the very Timbuctoo of art." "Madrid," says he, writing to Sir Robert Peel, "is quite a mine of old pictures of which in England we know nothing." Stirling's mind was admirably adapted to fit him to be the explorer of this mine. Accordingly, the first fruits of his travels and studies in Spain were given to the world in 1848 in his "Annals of Spanish Painters." Sir Edmund Head had preceded him by a year in the publication of his "Handbook to the History of the Spanish and French Schools of Painting," and Stirling's book came, as it were, to clothe the dry bones of Head's work with living flesh. It scattered over the dull details of biography anecdotes bearing on the manners and customs of the different epochs which his history brought under his eye. It gives in some sort a glimpse at the history of the Spanish people. The book was received with enthusiasm, and placed the author at once in the front rank of art critics. It was a great success, but it must be confessed that it was a success in a narrow field; and had the author rested on his laurels he would have ere now dwindled into a comparatively small figure among his contemporaries. Fortunately for us, his intercourse with Spain and Spanish story, and his accurate history-loving mind, brought him into contact with a hero congenial to his tastes, with whose career, as a Scotchman and an admirer of the brilliant pages of Robertson, he must have been familiar from his youth—Spain's greatest or second greatest name, Charles V.

But Mr Stirling was no hero-worshipper; and when he follows Charles into the cloister, it is with no intention of painting him with the halo of a saint, but with the stern yet noble features of a man who, having adopted as his motto "Plus ultra," thus striking the negative from the limits of his ambition, paused in mid career that he might look out from the quiet eminence of his tower at Yuste, and witness, undisturbed by its noise, the working of the vast machine himself had set in motion.

"The Cloister Life of Charles V.," Mr Stirling's greatest work, originated in this way. A MS., entitled "Memoir of Charles at Yuste," had been deposited in the archives of the Foreign Office at Paris. Mr Stirling, anxious to solve some question in Spanish history, went there in the summer of 1850, and endeavoured in vain to get a sight of it. Nothing daunted, he returned in the

winter, backed by Lord Normanby, and finding favour with the President of the Republic, he was, though not without great difficulty, at length permitted to peruse the precious MS. He found it a real treasure, based as it was on the correspondence of the Emperor Charles from his place of retreat, with the courts of Valladolid and Brussels. The information derived from the perusal of this MS. (which he was not allowed to copy) supplied the groundwork of Mr Stirling's volume, on which, from his ample resources in Spanish literature, he founded the true story of Charles's cloister life, so different from the life depicted in the charming pages of Robertson. We see the Emperor setting out from Flanders in a truly imperial manner, accompanied by his two sisters, queens of Hungary and France, with a train of 150 followers and a fleet of 60 sail. We see him arrived at Burgos amidst the pealing of bells and the shouts of the populace. We follow him threading the mountain passes of Spain till he reaches the castle of the Count of Oropesas, sick and worn out, not with fasting and prayer, but with excessive indulgence at table. We hear of the whole country-side aroused at his advent, and we have not long to wait till we find every pass which leads to his retirement threaded by strings of sumpter mules laden with everything which nature and art could find out to administer to Charles's inordinate love of good cheer. We see him at length in his cell at Yuste—a cell which had been three years in its construction, built especially for his use—with sixty attendants only, feasting right royally every day, and in all respects (if Mr Stirling has not somewhat overstated his case, which is more than probable) as much a man of the world and an emperor as he had ever been at Valladolid or in Brussels. To his countrymen (who had been enamoured with Charles's cloister life from reading of it only in the fascinating pages of Robertson) the effect produced by Mr Stirling's book must have been somewhat analogous to the deletion of one of the saints from their calendar.

Mr Stirling having completed for the present his historical studies, returned to his first love—Spanish art, and three years later he published "Velazquez and his Works," a singularly able monograph.

It is not necessary in this place to notice the works which were privately printed at his expense, mostly illustrative of Spanish history and art. It is to be regretted that their circulation was

limited to a few friends and to the public libraries, so as to be nearly inaccessible to the general reader; nor is it my duty more than to mention the production, in a splendid form, of the history, by Mr W. Fraser, of the Stirlings of Keir. Lord Houghton informs us that at the time of his death Sir William had announced the speedy completion of the "Life of Don Juan of Austria."

Mr Stirling received academic honours from nearly every University in Britain. In particular, he had been elected Lord Rector of the University of Edinburgh, and at his death he was Chancellor of the University of Glasgow. The rectorial address which he delivered to the students in Edinburgh is characteristic of the man. It recommends caution in forming an opinion, hesitation in choosing a party, whether in politics or in religion, and, above all, sober-mindedness. I quote one passage:—"Let me recommend to your notice the rule of Descartes—the first of the code which he composed for the guidance of his own mind—'Never to receive anything for truth which I do not clearly know to be true—that is, carefully to avoid haste and prejudice, and to include in my judgment nothing which does not present itself so clearly and distinctly to my mind as to take away all occasion of doubt.' If any considerable number of men could be induced to walk by this rule, how blessed a calm would descend upon many places now filled with noise and confusion! how many of our intellectual battle-fields would be left with 'their lances unlifted, their trumpets unblown,' ready for the ploughshare of profitable industry! how much speech, which can be hardly called even silver, would be hushed in a happy and golden silence!"

Sir William died of fever at Venice on the 15th January.

Sir William Gibson-Craig, the eldest son of Sir James Gibson-Craig of Riccarton, Bart., was born on the 2d of August 1797. He traced his descent to Sir Alexander Gibson, who was President of the Court of Session in the reign of James VI., and a daughter of Thomas Craig of Reccarton, the celebrated jurist of that time, and author of the treatise *De Jure Feudali.* Having been educated at the Edinburgh High School, and afterwards at a school in Yorkshire, he was called to the Scotch bar in 1820; but instead of settling down to a forensic career, he spent two years in foreign travel, and on his

return to Edinburgh, turned his attention to public affairs. He took an active part in Church matters, and on the dissolution of Parliament which followed the accession of Queen Victoria in 1837, he offered himself for the representation of his native county, Mid-Lothian, and was returned by a majority of 42. Four years later he retired from the representation of the county, and offered himself for that of the city of Edinburgh. He was returned as the successful candidate along with Mr Macaulay, and retained his seat till 1852, having held during six years of the period the post of a Lord of the Treasury, in which position he had endeavoured to take a real charge of the affairs of Scotland. To him perhaps more than to any other man we are indebted for the erection of the National Gallery. In 1862 he was appointed to the office of Lord Clerk Register and Keeper of the Signet in Scotland; and in the following year he was sworn a member of Her Majesty's Privy Council. For many years Sir William performed the duties of this office of Lord Clerk Register gratuitously. Subsequently, on the recommendation of a select Committee of the House of Commons, which took evidence on various matters connected with the Register House, the salary of £1200 a year, which had formerly been attached to the office, was restored about 1871. Sir William exerted himself to render the working of the Register House efficient, and in particular to carry out the recommendations of the committee above referred to. Amongst other objects which engaged his attention was the publication of many of the interesting documents contained in the Register House. One undertaking he thus helped to forward was the index volume to Thomson's Acts of Parliament prepared by Professor Cosmo Innes, and the recasting of a portion of that work, with the addition of Acts discovered since Thomson's time. Another was the collection of Privy Council Records, edited by Mr Hill Burton, of which the first volume has been published. For many years Sir William acted as Chairman of the Board of Visitors of the Royal Observatory, on which Board I had the pleasure of sitting with him. Here he brought to bear his knowledge of the forms and usages customary in approaching the Treasury, and he was a steady and efficient supporter of the Astronomer Royal in his endeavours to get the Observatory put on a proper footing. To all connected with him on that Board his conduct was ever that of a high-minded

and courteous gentleman, and his retirement was a source of deep regret to all his colleagues.

Sir William died on the 12th of March, in his 81st year.

Martyn John Roberts, born August 2, 1806, was the only child of John Roberts and Caroline, the daughter of William Yalden, of an old Hampshire family. His father resided at "Bryn Caeran, Caermarthenshire," and did not think it necessary to bring up his son to any profession or business, but he soon made himself acquainted with all the mining and other industrial occupations in the district. Early in life, he manifested a great distaste for mere classical learning, and an immense avidity for scientific pursuits; and as science was not to be acquired at school or at home, he found out every thing for himself while discouraged by all those around him. Amongst the Welsh servants and peasantry, he was considered somewhat in the light of a magician, owing to the extraordinary experiments he was always making; and in thus teaching himself, he often rediscovered many facts already known to others with whose works he could not then become acquainted. But had he heard of them he could not have been satisfied without proving them. He had a rare combination of exact scientific research with flashes of imagination, and thus the study of electricity always possessed a peculiar charm for him. There, he saw a very imperfectly explored and wonderful region with plenty of room for experiment and discovery.

He was never all his life without a carpenter and smith working out his ideas, so needful was it for him to create a tangible representation of them, and so unable was he with his refined, nervous fingers to do any rough manual work for himself. Both his parents died before he was of age. He began his independent career by having a yacht built on his own plan, and sailed about to visit various places abroad, making acquaintance with Cuvier, Clement-Desormes, and other scientific men in Paris. Thus he came in contact with all that had been already done in science, and knew over what field his future investigations must range. In this way also he made himself thoroughly master of navigation, and was qualified to suggest improvements regarding ships. Hence, we find him, July 9, 1838, receiving thanks from the United Service

Museum for a new form of anchor deposited there; and early in 1839, a medal was conferred on him by "the Society of Arts of Scotland," for his new method of reshipping a rudder at sea, an account of which was published in the *Edinburgh New Philosophical Journal*, No. 53. Some of his contributions on the subject of electricity may be briefly noticed, but as his thoughts were sown broadcast by extempore lectures and in newspapers, it is impossible to do more than allude to them generally.

He always combated the notion of inherent repulsion, whether in gravity or electricity, and as, by independent research, the phenomena of electricity gradually opened out to him, he became convinced that gravity was dependent on an electric condition, and that there were not two electric fluids, essentially positive and negative, repellent and attractive, but only one fluid, either "plus" or "minus" in quantity or intensity, and always attractive. He maintained that apparent repulsion could be explained by one attractive fluid, always tending to equilibrium, and he wrote and lectured much to account for the phenomena generally supposed to be adverse to his theory. The papers published in the *London and Dublin Philosophical Magazine*, vol. xix., July 1841, and vol. xxi., 1842, show that while studying anatomy at the Edinburgh University in 1838, he was collecting materials to prosecute his favourite researches in electricity. It was chiefly but not entirely with reference to these papers, that the late Dr Grant (professor of comparative anatomy at the London University) wrote as follows:—"In the whole range of my experience, no one surpassed Roberts for deep and original views in practical chemistry and physiology."

He initiated several forms of galvanic battery. The first mentioned by him, he applied more especially to the assaying of copper ores in October 1837, also to the blasting of rocks; and in September 1838 he sent papers on both subjects to the Royal Geological Society, Cornwall, describing his method of effecting those objects. It was on that occasion that he was unanimously elected Member of this Society. The following November a letter from him, explaining the process, was read by Mr J. P. Gassiot at the London Electrical Society, of which he was one of the earliest members. Another letter, dated March 1839, was read at the same Society, describing a new battery in which the metals were circular discs, and were

arranged on a horizontal axis, so that by turning a handle they might be rotated. There was an apparatus for cleansing the plates as they turned, and additional galvanic energy was obtained after exposure to the air.

That was the battery used in blasting the Skerryvore rocks. Other papers of his are to be found in the "Proceedings of the London Electrical Society," 1837–40 and 1843, pp. 77, 78, 356–60, "On Radiation not a property of Electricity," &c., &c. To conclude the subject of blasting, it may be as well to say that on the 26th March and 26th April 1838 (at the request of the Highland and Agricultural Society of Scotland) he exhibited, not only his process of blasting rocks, but that of explosion under water, at Craigleith Quarry, for the perfect success of which he received a medal from the Society. A drawing of the scene, showing the height to which the water rose, was made at the time, and afterwards engraved. Some sappers and miners, then present, being taught the process, made it known to Colonel Pasley, and the result was his blowing up the "Royal George." It was not, however, until 1840 that (by desire of many) he published a pamphlet, giving simple, practical directions for general work, and detailing the use he had made of sand for tamping, and the introduction of atmospheric air between the charge of powder and the sand, so as to increase the energy of explosion. He first pointed out that sand so placed in a tube could not be blown out. In the "Proceedings of the Highland and Agricultural Society" may be seen the original paper, of which the pamphlet was a copy, and it was still further illustrated in the "Proceedings of the West Yorkshire Geological Society" for 1842–48, pp. 126–138. The last act of the kind he personally superintended was in a quarry near Rydal, to please the poet Wordsworth and Dr Davy. In the "Mining Journal" a controversy was begun 18th January 1841, and ended by two letters—one from Mr Byers, stating that he had made much practical use of assaying ores by galvanism, the idea of which originated with and was communicated to him by Mr Roberts; the other was from Mr Roberts himself, showing that M. Becquerel's essay "On Detecting Metals in their Solutions" did not bear on the subject, as he did not profess to give any practical method for detecting the quantity and variety of metals contained in the ore.

Mr Roberts set out on a scientific expedition to Norway in the

summer of 1839, but was detained for some time at Amsterdam by rheumatic fever, and obliged to return home from great prostration. Throughout many years of his life he suffered from frequent attacks of the same kind, often checking him in the midst of work. However, he was not long after this ready with a paper "On an Anomalous Condition of Iron." Experiments were detailed, showing iron to be positive when compared with copper, and yet far more highly negative than copper when compared in their electric relation with zinc. The result of this newly discovered fact was, that he recommended to the Admiralty a new method of sheathing ships, and made known the use to which it might be applied in the manufacture of boilers.

In 1851 he contrived a battery for obtaining products that might be utilised in the arts, and thus cheapen the use of electricity, but soon afterwards those products became so plentiful as to be of little commercial value.

For the same purpose, he recovered his products and used them over and over again without much waste. It is interesting, at the present moment, to read the description of an electric lamp, for which he then took out a patent, but which was not carried into general use for want of sufficient funds. The distance between the graphite or charcoal electrodes was regulated by a kind of clockwork, and two sets of electrodes used to produce equality of light, or else "the electrodes were placed in a vacuum, or space not containing any oxygen or other matter which could cause their destruction when brought into an incandescent state by the action of the current of electricity." The specification, with full drawings, dated 1852, No. 14, 198, may afford some hints for those at work in a similar direction now. It may not have been known to those who have lately proposed the same thing, but the coincidence is singular. While visiting the manufacturing districts of Glasgow and Leeds, he used his chemical knowledge to improve the making of paints and dyeing of woollen cloths, and also helped in reducing the friction of spindles. It should here be mentioned that, in the course of his life he took out several patents, but a mind so sensitive as his was not fitted for the wear and anxiety of commercial speculation, and the fertility of his thoughts would not allow him (like the man of one idea) to rest long on anything he

had done. He had scarcely constructed one machine before he shot right ahead of it, and thought of something better, which his honesty, and candour would not allow him to conceal. His last invention was a safety-valve most carefully superintended by himself, and being his last, it is to be hoped that those who have taken out a patent for it will be rewarded by a pecuniary profit he did not derive from anything for himself.

Wherever he stayed, he was one of the first to found and promote mechanics' institutions; and many were his lectures on science, freely given to working men, by whom he was, in consequence, much regarded, and from many of whom he afterwards received letters thanking him for the impetus he had given to their intelligent industry and prosperity.

When in the Highlands, he interested himself much in the condition of the crofters, and his suggestions on that subject were published in the newspapers of the day. The people of Fort William must remember his gathering them of an evening for rational amusement and instruction.

In 1854 his love of fine scenery induced him to purchase a picturesque property in Breconshire, where he built a house, and settled his family for eighteen years. The land, which he found full of stony hedgerows and swampy places, he made a verdant park, and planted it with many fine trees.

But the work in which he took most interest there was the administration of justice, as deputy-lieutenant and magistrate for two counties. He was not satisfied with a mere "dilettante" observance of his duty, but studied and mastered the laws on the knowledge of which he was to make a decision. As a visitor of the United Lunatic Asylum at Abergavenny, he always threw light on the subjects discussed, and his ideas were gathered up in pamphlets and periodicals that might influence the community in which he lived. Some of his suggestions respecting "the poor laws" were afterwards adopted. His "Plea for Pauper Children," advocating industrial and classified schools, was published in 1861. He also made a move against the injustice done to poor people by deficient weights and measures, and was called upon to give evidence on them before a Committee of the House of Commons, which led to a more stringent regulation. On that occasion, the Astronomer Royal showed his

power of memory. Hearing the name of "Martyn Roberts" he asked, "Did you as a boy write me a remarkable letter on astronomy?" It was quite true, and occurred some forty years before.

It would be impossible and unnecessary here to enumerate all his fugitive writings and the many things to which he turned his attention during his life, or to notice the testimony of those who knew his usefulness.

Through all his activity there glowed a very passion for the beauties of that extensive prospect at Pendarven and the glories of light and shade on the distant hills. This was to be expected in one ever looking out for perfection, and crying "Excelsior!" and often, while gazing on those splendid mountain sunsets, he was

> "Rapt into still communion that transcends
> The imperfect offices of prayer and praise.
> Of him it might be said that he was found
> By his intense conceptions, to receive
> Deeply, the lesson deep of love."

He dearly loved his home, and delighted in listening to the music of his family. Yet like most men of imaginative wit, he was not only full of genial humour, but excitable, and ready with slashing sarcasm for obstructive stupidity and dishonesty. His family and friends can testify to his affectionate and attachable disposition. Indeed, his heart was too tender to admit of his ever joining in the ordinary country sports. He took delight in the songs and flight of birds, and never could hurt or shoot a living thing for pleasure.

The last few years of his life were spent in Bath, where he underwent much suffering, until he died on the 8th September 1878. But his mortal remains rest beside those of his eldest son in the Welsh churchyard of Llangenny, where he had so often listened to the rush of the mountain stream, and felt that all was to him a revelation of the Divine.

Robert Harkness was born at Ormskirk, Lancashire, on 28th July 1816. He was educated first at Dumfries, and afterwards at the University of Edinburgh; so that although English by birth he was Scotch by early training and residence, and even to the last was often supposed to be a native of the north side of the Tweed. It was while attending the lectures of Professor Jameson that he

had his attention specially directed to geological pursuits, and the influence thus communicated moulded all his after life. His first paper, published when he was twenty-seven years of age, was a speculative one, on the climate of the coal epoch, and contained ideas which have been adopted by some subsequent writers. A second paper, on changes in the temperature of the earth as a mode of accounting for the subsidence of the ocean, and for the consequent formation of sea-beaches above its present level, was read by him in the same year before the Geological Society of London. But in spite of this early promise of activity in theoretical geology, it was as a sedulous worker in the field that he distinguished himself. Though he made occasional geological excursions in Lancashire and Cheshire, he spent most of his early years in laborious traverses of the south of Scotland, exploring the old reptiliferous sandstones of Nithsdale and Annandale, the Carboniferous Limestone as it is developed in Dumfriesshire, and more particularly the Lower Silurian formations which stretch from the coast of Berwickshire to that of Wigtown. To his minute observations we owe the first outline of the general structure of the Silurian uplands of the south of Scotland. He followed the graptolite bands from valley to valley, and from parish to parish, and showed how they could be used as horizons to determine the succession of deposits in that difficult region.

In the year 1853 he was appointed to the Chair of Geology in Queen's College, Cork—an office which he held till his death. His residence in Ireland enabled him to bring the same diligent zeal to bear on the investigation of the geology of that island. From time to time he made valuable contributions to our knowledge of Irish geology, one of the most important being the paper which he wrote in the year 1860, on the metamorphic rocks of the north of Ireland, wherein he drew a parallel between the quartz-rocks, limestones, and associated rocks of Donegal and those of the west of Scotland. But he continued to devote much time to field-work in the north of England, and in Scotland. There is hardly a district which he did not explore ; and though he did not always publish his observations, the number of his communications to the scientific journals of the day bears witness to his unwearied activity. He made himself the best authority of his time in the palæontology and stratigraphy of the Lake District and of Dumfriesshire.

The alteration of the curriculum of the Queen's Colleges in Ireland included a very great increase in the duties required of the professor of geology at Cork. Professor Harkness struggled with this accession of toil for two years, but finding it too much for his strength, and having some premonitory symptoms of the disease which ultimately cut him off, he resigned his chair, after having occupied it for a quarter of a century. It was his intention to settle at Penrith, to which place he had for many years been used annually to repair to spend a portion of his holiday with his sister. It was when on his way to carry out this intention that he died suddenly of heart-disease at Dublin, on 4th October 1878.

Important as was the scientific work accomplished by Professor Harkness, it did not receive a wider or heartier recognition among his brother geologists than his admirable qualities of head and heart. No one who has been privileged with his friendship will fail to cherish the memory of his earnestness over even the driest details, his quiet enthusiasm, his generous admiration for the work of others, and his unfailing cheerfulness. His beaming ruddy face, always full of kindliness, was seldom to be missed from the platform of Section C at the British Association meetings. It often rose among the speakers, and it never failed to reappear at the festive evening gatherings. There have been men who have graven their names more deeply on the registers of scientific thought and progress, but there have been few whose sunny nature has more endeared them in the recollection of their friends than Robert Harkness.

A few words in conclusion.—Just twenty years have elapsed since I had the honour of delivering an address to this Society on its opening day. On that occasion, I took the place of Sir David Brewster, who was suffering from temporary illness. The feelings which arise on casting one's thoughts back through twenty years are full of sadness when they fasten on individual members of this Society, whose presence at our meetings was a source of pleasure not unmixed with pride, but of sadness, brightened by glimpses of the future when we think of them as members of a living body, as workers even now in the field which man has been sent into the world to cultivate—the field where truth is to be sought and found.

Great men like Sir David Brewster are not all lost to us; they live

and work in the impetus they have given to younger men, who hung about them when living, and fill their places when dead. This feeling is forced on me very powerfully by one fact which it was my duty to state in my former address. It was this. I said—"The annual addition to our Transactions this year (1857–8) contains but one paper. That paper is by Mr Stewart, and is of unquestionable merit. I have great pleasure in learning that Mr Stewart is continuing his researches on radiant heat, a branch of experimental science which owes so much to members of this Society, and the papers on which alone suffice to stamp our Transactions with lasting value." I need not say that the papers to which I referred were, for the most part, contributed by one member of the Society, the man who had for the previous twenty years of my connection with it most conspicuously illustrated in his own person the position which he himself lays down, that the vitality of a Society like this is kept up by a few ardent workers, whose contributions like the life blood trickle through the veins of the Society, and warm and animate its remotest members. Forbes was a man pre-eminently constituted to be a leader in a Society like this—cold, yet thoroughly affable; always at work, yet always accessible; keen and resolute in maintaining his own claims, yet ever open to the claims of those about him; and above all, straightforward even at the risk of treading on the toes of men with whom he might happen to come in contact. Forbes was one of two members of the Society whom I had the honour of knowing before I came to Edinburgh. He was introduced to be by Dr Whewell, and was ever my fast friend. Though perhaps a little out of place in such an address, I cannot resist the inclination, in holding up his ardent, hopeful character to the younger members of the Society, to read a portion of a letter which I received from him on his last return from the Continent to lie down and die. It is written in bed and in pencil. He says—

"I was very glad to hear from you. You will not expect a long reply. I am, in fact, very weak, though I made out the journey wonderfully, by the help of every luxury and indulgence which modern railways and hotels afford. Here on English ground I am content for the present to rest and be thankful; to leave the issue to a merciful Providence, in whose goodness and guidance I place my full reliance. Of course, I do not look at present to any further movement.

It is most gratifying to me to know that you and other old friends still thought of me as a successor to Sir David Brewster [in the Principalship of the University]. *That,* of course, I at once surrendered. It was the last remaining rag of worldly ambition which remained to me, and I surrendered it cheerfully."

I have selected Forbes out of the many who are called up to memory by a reference to what I said in this place twenty years ago, both because he is most vividly associated in my mind with this room and this Society, and because he is about the very best type I could select of a man who derives benefit from the associations connected with a Society like this, and who in his turn reflects those benefits most powerfully on others. The solitary paper which I have mentioned as the sole product in our Transactions of the Session, was an early product of a mind lit up by a spark from Forbes's anvil. Balfour Stewart had been a worker in Forbes's workshop, and had imbibed much of the spirit of his master. He is now one of our Honorary Members; a fact which sufficiently expresses the opinion of this Society of the manner in which he has been doing his work. It would not become me to pursue the subject of the influence of one mind upon another, due to their close contact, by singling out some of the fervent workers in this Society as the insensible creations of the good men who have lived before them. The fact is patent. Good men have raised up good men to succeed them. Our Transactions of the present period contain papers not a few destined to take their place in the permanent repertories of science. We have about us workers whose praise is wide spread, but this is not the place to sound it. The only word I can venture on as both encouraging for the present and hopeful for the future, is the remarkable number of young men who are just entering on their work. In the fasciculus of the Society's Proceedings just issued, I count not less than eleven names of young men just entering on their career of investigation. How many of them have caught their inspiration from contact with those older workers who have been long among us! How many have been drawn out and cheered on by the associations of this room!

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. X. 1878–79. No. 104.

NINETY-SIXTH SESSION.

Monday, 16th December 1878.

Professor KELLAND, President, in the Chair.

The following Communications were read :—

I. On the Action of Light on the Iris. By William Ackroyd, F.I.C., &c. Communicated by Professor M'Kendrick.

Sect. I. It is well known that the movements of the iris are due to the stimulus of light, but I am not aware that any experiments have been hitherto made to determine the approximate quantity of that agent necessary to bring about this involuntary action. The usual way of observation precludes refined experimenting, it being customary to watch the iris of another person or animal whilst under the influence of varying amounts of light, or one's own iris by means of a mirror. Three methods will be described here, and I believe that one at least may afford a means of getting new data on this and other points.

Sect. II. The first and second methods depend upon the following facts :—That, if a divergent bundle of rays emanate from a small surface or hole, very near to the eye (say about 30 mm. off), this surface or hole is the apex of a cone of light whose base is the pupil; that every movement of the iris affects the area of this base, which

appears as a circular luminous field; and finally, that I find these alterations of area, so easily seen, may be taken as indications of the movements of the iris.

The third method is equally simple. The lachrymal fluid on the surface of the cornea affects the image of any light source, such as a lamp or star, and by refraction causes the appearance of rays to emanate therefrom.

It is obvious that the length of these rays must be regulated by the iris, this organ being nearer to the retina, hence when the pupil contracts the rays ought to shorten, and when the pupil expands the rays ought to lengthen out. Such I find to be the case.

Sect. III. *The First or Reflection Method.*—The following is the simplest form of the experiment I have been able to devise. Burnish the head of an ordinary brass pin, and place the pin up to head in a black hat. Now, with one eye shut and your back to the light, bring this pin-head near to the other eye so that the light may be reflected into it from the convex surface of the pin-head.

One sees a circular luminous field, with projecting hairs at the bottom which belong to the top eyelid.* Globules of the lachrymal fluid also appear at each wink.

Expt. 1. Shade the light from the observing eye for a few seconds, then let the light fall on it again. Notice the alteration in area of the field of view. The field contracts, then expands slightly, and oscillates until the iris is adjusted for the amount of light falling into the eye.

Expt. 2. Observe the pin-head with the right eye for some moments, the left eye being closed. Open the left eye. The iris of the right eye is seen to move markedly, the pupil contracting. *Here the iris of the right eye is moved by the light entering the left one.*

Expt. 3. With everything as in Expt. 2, have both eyes closed and only open the right or observing eye. There is contraction of the pupil, but apparently no more marked than in Expt. 2.

* A simple method is here suggested for demonstrating to one's self the inverting action of the crystalline lens. With everything as here described, take a needle and bring it across the field of view *close* to the eyelids. *If it move downwards, it appears to move upwards; if it be moved upwards, it appears to come downwards.*

Sect. IV. *The Second or Transmission Method.*—Prick a pin-hole in tinfoil. Shut one eye and bring the hole within 12 mm. off the open eye.

Expts. 1, 2, and 3 may readily be repeated by this method.

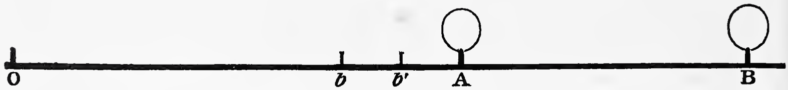
Expt. 4. Place green glass before the aperture, and notice the size of the field, then withdraw the glass suddenly. The pupil contracts. Red glass gave the same result.

Sect. V. *The Third or Refraction Method.*—The following example will make perfectly clear the way of working here:—I am looking at a star, with the moon at full, a little to one side. From the star proceed the rays mentioned at the close of Sect. II. Upon turning towards the moon, but still keeping my attention concentrated on the star, the rays of the latter appear to retreat into it; and upon turning from the light of the moon, the rays emanate from the star again.

Expt. 5. This is typical of about seventy other experiments I have made. The night is starless. An isolated gas lamp, with no houses near or any other sources of light, appears when seen from a distance with the usual rays emanating from it. I walk towards it slowly. At 300 yards no alteration has taken place in the rays; they appear fixed. The distance is slowly decreased, but not until I am at a distance of 16 yards do the rays perceptibly shorten; in other words, the light from this one gas lamp is incompetent to effect a movement of my iris until I am within 16 yards of it. The shortening of the rays is now rapid, for at 10 yards distance the light appears to be without them.

Expt. 6. In the preceding experiment there is a possibility that the rays may be shortened to some extent by the increase in size of the image on the surface of the cornea as we near the light. In the present experiment this objection is to some extent removed. Two gas lamps were chosen, 50 yards apart, and whilst walking towards the nearest my attention was kept exclusively on the rays emanating from the farthest one. As the first lamp is approached, the effect of its light on the iris is visible in the alteration of length of rays proceeding from the far one. Thus in the fig. the two lamps are A and B, and the observer stationed at O sees rays emanating from both. A is the lamp whose influence on the iris is to be tested, and B is the lamplight used as a tester. Proceeding from O

towards B, a point b is reached at which the lamp rays of B begin to shorten, *i.e.*, the light of A affects the iris. Getting nearer still



to A, a point b' is reached, where the distant light B appears to have lost its rays.

The average of a dozen experiments gave as the value of bA... 14 yards, and as the value of b'A... 8 yards. Squaring these numbers, it appears that about one-third of the light competent to contract the pupil very markedly is sufficient to start its movement.

At present, I abstain from comment, as I am continuing these experiments.

2. On a New Variety of Ocular Spectrum. By John Aitken.

If we look for a short time at any object, and afterwards turn the eye in another direction, we see a spectral image of the form of the object first looked at.

Again, if after we have looked at any coloured object we turn the eye in another direction, we see a spectral image of a colour complimentary to that first looked at.

In addition to these spectral forms and colours, I find there is another and distinct kind of ocular spectrum, which we may call a motion spectrum. It is seen when we look first at a body in motion and afterwards direct the eye towards an object at rest. The object at rest, when seen under these conditions, seems to be in motion, and the direction of its apparent motion is the opposite of that of the moving body first looked at.

I first observed this motion spectrum when looking at the surface of a river where it was flowing rapidly, the eye being afterwards directed to a gravel bank. The first effect seemed to be an indistinctness of vision, but, on carefully repeating the experiment, I was much astonished to observe that, after looking steadily at the stream and then at the gravel bank, a narrow spectral stream of gravel seemed to flow steadily through the middle of the gravel bank, the direction of its motion being the opposite of that of the

river. Sir D. Brewster made some experiments while looking out of rapid-moving trains on the effect of images moving rapidly across the retina, but he does not seem to have observed these motion spectra. Professor Silvanus P. Thompson has, however, observed a somewhat similar phenomenon.* He says:—"If, from a rapid railway train, objects from which the train is receding be watched, they seem to shrink as they are left behind, their images contracting and moving from the edges of the retina towards its centre. If, after watching this motion for some time, the gaze be transferred to an object at a constant distance from the eye, it seems to be actually expanding and approaching."

Under ordinary circumstances motion spectra are not easily noticed. Like form and colour spectra they require special precautions to be taken before they can be distinctly observed. I find, however, that these precautions are extremely simple, and that but little apparatus is required for showing them distinctly.

Simply take a circular cardboard disc,† painted as shown in fig. 1, and mount it on a horizontal shaft. Now, if, while this disc is being steadily rotated,‡ the eye is fixed upon it, and the motion continued a short time and then stopped, the disc will at once appear to begin rotating in the opposite direction. The experiment is, however, improved by painting either a copy of the disc, or a wheel, or any other object, on a sheet of paper, and hanging it up near the rotating disc. If, after looking at the rotating disc

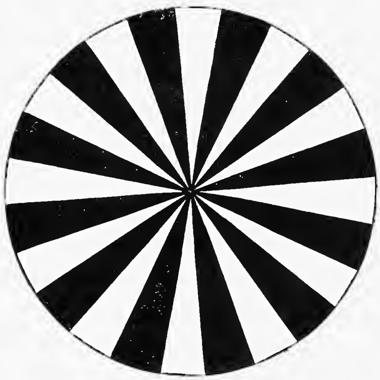


Fig. 1.

* Report of the British Association, 1877.

† The discs used in the experiments were from 22 cm. to 44 cm. in diameter. The number of parts into which the disc is divided does not seem to be of much importance. If divided into fewer parts, then it simply requires to be driven at a greater velocity. If divided into 24 parts, it can easily be driven quick enough with a handle on the opposite end of the shaft without the aid of multiplying gear.

‡ The disc should be rotated at such a rate that the eye cannot distinguish the black and white divisions, but not so quick that they are blended together.

for a short time, the eye is directed to the drawing, the wheel, or whatever it is, appears to be rotating in a direction the opposite of that of the exciting disc. Or we may vary the experiment, and look at a sheet of paper having a mottled surface.* When we do so, after looking at the rotating disc, we see a circular spectrum of the disc, in which the markings on the paper no longer appear stationary, but all seem moving in a circular direction, like a slow-moving whirlpool, the direction of the motion being the opposite of that of the exciting disc.

In these cases the motion was circular, and the result was a circular spectral motion. We may, however, vary the experiment, when we shall find that the spectrum will always correspond to the exciting motion. Suppose we take an endless band of paper, with black bars painted across it, and pass it over two drums revolving on horizontal shafts, one placed over the other, so that the paper band shall move in a straight line either upwards or downwards. If, while this band is in motion, the eye be fixed upon it for a short time and then the gaze be directed to the mottled paper, a spectrum of the moving band will be seen. A narrow strip of the mottlings will appear to flow through the mottled sheet of paper, reminding one strongly of the appearance of a lava stream, the breadth of the stream corresponding to the breadth of the moving paper band, and the direction of its apparent motion being the opposite of the moving band first looked at.

Or we may vary the experiment in this way:—Take a wheel, having spiral spokes coloured black, and rotate it in front of a disc similar to that first described, but in this case kept stationary, so that the white parts seem to travel from the centre to the circumference, or from the circumference to the centre of the disc, according to the direction in which the wheel is turned. If the mottled paper is looked at after looking at the apparatus in motion, all the mottlings in the spectrum seem to be in motion, either towards the

* The markings on the paper should not be too strongly contrasted with the paper. White paper roughly spotted over with ink will do, but the effect is greatly increased when the contrast is not so great. The best effect is produced by first washing the paper all over with Indian ink, thick enough to make the paper a darkish grey, and, while still wet, daubing it all over with Indian ink. In the absence of anything better, a cocoa-nut fibre mat does well.

centre or the circumference, according to the direction of the motion of the real impression.

These motion spectra are also seen if the eyes are closed after looking at the moving body, the spectrum of the moving paper band suggesting a phantom shower of rain in sunshine, the direction of the apparent motion being the opposite of that of the real impression.

It might be thought, since the spectrum of the moving band seen on the mottled paper seems to be in motion, and as some of the mottlings seem to flow past the others, that if we were to draw a straight line across the spectral stream, the line ought to appear bent, because it might be expected that the part of the line in the stream would appear to move forwards. Such, however, is not the case. So far as my experiments go, I have never seen the least appearance of a bend produced in a straight line; indeed, the straight line does much to stop the apparent motion. Again, in the first experiment with the circular disc, if we make the drawing at which we afterwards look larger than the exciting disc,—as, for instance, by extending the spokes of the wheel to a greater size than the rotating disc,—then this extension will entirely destroy all appearance of rotation, and the wheel will appear at rest. Do not these last experiments suggest that the seat of the illusion is deeper than the retina? I shall not, however, attempt to answer this question, as the experiments do not point to any definite conclusion.

Experiments were also made to determine the effect of influencing the whole retina. This was done by looking so closely at the moving band that its image covered the whole retina, but no decided effect was noticed. Experiments were also made with the same object by means of a large box-shaped arrangement, the sides of which were made of tracing paper having vertical bands of black paper 4 cm. broad and fixed 4 cm. apart. The observer being seated in a chair, the box was let down over him and put in motion, which was continued some time; the box was then raised, but no appearance of motion in surrounding objects was observed. There were, however, some curious effects produced by the rotation of this apparatus. At certain times, while surrounded by the rotating box, the observer felt as if rotating in the opposite direction. The most certain result, however, was a most disagreeable sickening effect,

which continued for some time after the experiment was made. The effect of this rotating apparatus might form an interesting study in connection with the important investigation at present conducted by Professor Alex. Crum Brown on the function of the semicircular canals of the ear.

3. On the Principles of the Logical Algebra; with Applications. Part I. By Alexander Macfarlane, M.A., D.Sc.

(Read 16th December 1878.)

(*Abstract.*)

In this memoir I examine the principles of the logical calculus of Boole, as laid down in his celebrated treatise on the "Laws of Thought," and also the criticisms which have been published concerning these principles. I bring forward a new theory of the operation of the mind, founded upon an analysis of language and the nature of mathematical reasoning, which enables me to correct these principles, to place them on a clear, rational, and generalised basis, and to show that there is a logical algebra which coincides with the ordinary algebra when its symbols are integral, but is a generalised form of the ordinary algebra when its symbols are fractional. Hence all the theorems in ordinary algebra when generalised properly are true in the logical algebra. I show the analytical meaning of the axioms of logic and their relation to the algebraic axioms of operation. By means of this algebra I investigate the theory of immediate inference, and also the conclusions and numbers of conclusions of different kinds which can be deduced from premises of certain given forms. The memoir also professes to prove a great number of new theorems in the theory of necessary and probable inference.

4. Note on Ulodendron and Halonia. By Mr D'Arcy Wentworth Thompson. Communicated by Sir Wyville Thomson.

Monday, 6th January 1879.

Professor KELLAND, President, in the Chair.

The following Communications were read:—

1. Notes on some Experiments with the Telephone. By James Blyth, M.A.

While experimenting with an ordinary Bell telephone, of small resistance, I found that it was able to reveal the existence of electric currents produced by the mere friction between conducting substances. This was shown in the following way. Two files had wires firmly connected to them, and were thereby attached to the terminals of a telephone circuit in a distant room. When these files were rubbed against each other, a most distinct grating noise was heard in the receiving telephone. In order to find if this sound varied, when different substances were rubbed together, the following plan was adopted. A wire was firmly attached to a small table-vice, and led to one of the terminals of the telephone circuit, while the wire from the other terminal was attached to a clamp into which any substance, which it was desired to test, could be screwed. Different substances were then screwed into the vice and clamp, and rubbed against each other by an assistant, in each case, as far as possible, with the same pressure. By listening attentively in the receiving telephone, I endeavoured to detect any variation in the sound as the assistant passed from one substance to another. As far as I could judge, little or no variation was produced when the following substances were rubbed on themselves and on each other, viz.: steel, brass, iron, zinc, lead, gas carbon, copper, with possibly the exception of copper on iron, which, I thought, gave the sound a little louder.

It was different, however, when two pieces of antimony and bismuth were rubbed together. In this case the sound was decidedly louder. I tried also, with a very distinct effect, antimony rubbing on gold, and antimony on silver.

In order to augment the currents, and consequently the sounds produced in this way, I took a large iron fly-wheel mounted on an axis which ran in centres. By a wire attached to one of the centres this wheel was connected to one of the terminals of the circuit, while a file was connected to the other terminal. The wheel was then driven rapidly round, and the file held hard on to its rim,—so hard that sparks of fire were produced by the friction. In this way a very distinct noise was heard in the receiving telephone. I have also made a variety of the above experiment by mounting a small cylinder of antimony in a turning-lathe, and driving it round against a bar of bismuth. This produces the loudest and most distinct noise of anything which I have yet tried.

These experiments demonstrate, without doubt, the existence of currents produced in conducting substances by friction alone, but it becomes a question whether they are to be regarded as merely thermo-electric, or whether they are not the very currents referred to by Sir William Thomson as the probable cause of friction, and by Professor Tait, in his "Thermo-dynamics," where he says, "it is possible that all friction, not excepting that caused by actual abrasion, is due to the production of electricity."

Instead of rubbing the substances together, I next proceeded to try the effect of knocking the one against the other. For this purpose a small anvil was put into metallic connection with one of the terminals of the circuit, and a hammer similarly connected to the other. Each stroke of the hammer on the anvil was very faintly heard in the distant telephone. As a variety of this experiment I put a small quantity of detonating powder on the anvil, and came down upon it with a blow from the hammer. I thought that it might be possible to hear something of the sharp snap produced by such a blow. The sound, however, heard in the telephone was not appreciably louder than before. Another variety of this experiment was made by driving a wheel, with large teeth, rapidly round in the turning-lathe, and holding against it a strong metal spring. The rapid clicks produced in this way were heard even when the telephone was a short distance from the ear. Here, however, it is plain that we have a mixture of the effects produced by rubbing and knocking.

In my next experiment I took a phonograph, and so arranged it

that a telephone circuit was completed through the spring which carries the pricker, the pricker itself, and the cylinder. When the pricker was allowed to press hard into the groove, and the cylinder turned, a faint grating noise was heard in the telephone, unless at those points where there happened to have been regular serrate markings left by the tool in cutting the groove, and then, as the pricker passed over these, a sound more or less resembling a feeble attempt at an articulation was heard. I then put a sheet of tinfoil on the phonograph cylinder, and spoke a sentence loud and distinct into the mouthpiece, and, for the purpose of increasing the sound, as heard in the telephone, I also included two Bunsen's cells in the circuit. When the phonograph was now turned, so as to reproduce the sentence, the articulation was heard in the receiving telephone, loud enough certainly, but considerably marred by the mere rasping of the pricker on the natural inequalities of the tinfoil.

It is obvious that, in this experiment, the articulation, such as it is, heard in the telephone must be caused by the variation in the resistance to the current, which arises from the unequal pressure of the pricker upon the tinfoil as it follows its indentations. This has, I think, an important bearing upon the character of all curves got by different processes of enlargement from the tinfoil record. Such curves could only accurately correspond to the movements of the disc which produced the indentations, provided the style attached to the lever, for producing the enlargement, pressed exactly on the tinfoil as the pricker did. Now, seeing that the pricker does not press equally at all times on the tinfoil, it would be very difficult, if not impossible, so to arrange a style and enlarging lever as to press in a manner so exactly similar.

The telephone can be employed to illustrate, in a very pleasing way, the incipient stage in the breaking-up of a liquid vein into globules. For this purpose a vein of acidulated water is made part of a telephone circuit, which also includes one or two Bunsen cells. This is easily managed by attaching a metallic can, having a small orifice in its bottom, to one of the terminals of the circuit, and a shallow metallic basin to the other. The first vessel, being now filled with acidulated water, is held over the basin, so that the column of water from the orifice flows into it, and so completes the electric

circuit. By gradually raising or lowering the upper vessel, a longer or shorter column of liquid can be made part of the circuit. On listening in the telephone, so long as the liquid vein is short and limpid, no sound whatever is heard. This shows that the electric current has uninterrupted circulation. On gradually lengthening the liquid vein, a point is reached when a rattling noise is heard in the telephone. This arises from the altered resistance caused by the liquid vein beginning to break up into globules. On still farther lengthening the vein, a point is very soon reached when all sound in the telephone again ceases. This corresponds to the stage when the liquid vein has actually separated into detached drops, and so broken entirely the electric circuit.

2. On the Measurement of Beknottedness. By Professor Tait.

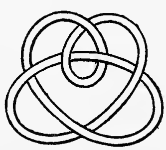
(*Abstract.*)

In my former papers on the subject of Knots, I have provisionally measured *Beknottedness* by the smallest number of changes of sign at the crossings, which will render all the crossings nugatory.

Though I have not seen occasion to doubt the accuracy of this mode of measurement, there are two objections to it—(1) It is very difficult of application in complex cases; (2) It suggests no direct relation to the electrodynamic method which, except in the case of knots wholly or partially amphicheiral, gives results quite in accordance with it.

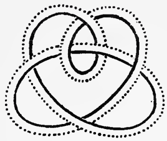
The object of the present paper is to describe a method which, while at least partially meeting these objections, very considerably simplifies some of the more important processes for the treatment of knots, which I have already given.

In this abstract a very simple example will suffice to indicate the method. Take the following six-fold knot



and modify the sketch, as on next page, the dotted line being traced always on the *right-hand side* of the full line as we go round the curve.

[In practice, the dotted line may conveniently be drawn with a coloured pencil or crayon.]



A little consideration shows that—

1. Of the four angles at each crossing, one is enclosed between full lines, and its vertical angle by dotted [coloured] lines. These will be called the *symmetrical* angles.

2. The crossing is electrodynamically positive if it is *over to the right* in the *symmetrical* angles, and *vice versâ*.

[In the figure the two interior crossings alone are positive.]

3. If the knot be cut through along a line dividing the *symmetrical* angles at any crossing, and the pairs of ends on either side of that line be reunited, the whole remains a knot, with one crossing less than before (Proc., 1877, p. 322). If the line divide the *unsymmetrical* angles, the whole becomes a link.

[Dividing the figure at the upper crossing, it becomes either the twist of five-fold knottiness, or the trefoil knot once linked with a simple ring.]

These methods are in practice very much superior in convenience of application to those I have already given, especially when the knot to be reduced is complex.

The paper contains rules for the calculation of the beknottedness of the original knot, in terms of the beknottedness and belinkedness of these reduced forms; so that knottiness n is made to depend upon $n - 1$. I have not yet succeeded in obtaining from these a general expression such as will take account of all the successive reductions of a knot to zero of knottiness.

3. Preliminary Note on the Measurement of the Thomson Effect by the Aid of Currents from the Gramme Machine. By Professor Tait.

4. On the Disruptive Discharge of Electricity. By Alexander Macfarlane, D.Sc., and P. M. Playfair, M.A.

(*Abstract.*)

During the months of November and December of this Session we have investigated certain questions suggested by the results already communicated to the Society.

Difference of Potential required to pass a spark between (1) *two equal spherical balls at different distances,* (2) *a plate and ball at different distances, and* (3) *a plate and point at different distances.*

A series of observations was taken for each of these, and on three successive days, without altering the arrangement of the apparatus or the charge of the electrometer. The couple of small Leyden jars were attached to the conductors of the Holtz machine, as we had previously found that it was impossible to observe the discharge between a plate and point with any degree of accuracy when the capacity was small.

Two Balls, each of $\frac{2}{3}$ inch diameter.—The series of observations for the two balls is a more minute and extended investigation of a problem we took up and solved approximately before. We have observed more minutely the values of the readings at the smaller distances, and also noted the cause of the irregularity at the ends. We found that at 80 mm. small violet sparks began to pass before the principal white spark, and that the reading was then more ambiguous than for smaller distances. Escape from the conductor was first noticed at 120 mm.*

Plate and Ball.—We employed a tin plate 8 inches diameter, and one of the brass balls used in the previous experiment. The curve obtained is not very different from that for the two balls; it is somewhat more circular. Small sparks passing before the large one were observed to begin at a shorter distance than in the previous case. Another irregularity at the end was due to the passing of two large sparks. Finally, the electricity began to escape from the insulated wires.

Plate and Point.—The plate used was the tin plate of the pre-

* Hence the irregularity previously observed is not due to the escape of electricity into the air, but to the passage of small sparks between the electrodes.

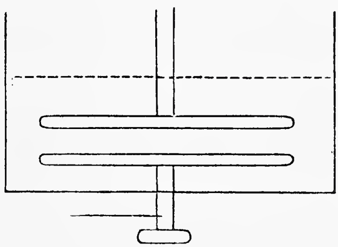
vious experiment; the point was conical and of brass. From 1 to 5 mm. the discharge was in the form of a white spark; for higher distances nothing was visible excepting a glow at the point. The series was continued up to 200 mm., as there was no difficulty due to escape of the electricity into the air.

Discharge through a Solid Dielectric.—We obtained, by favour of Mr Calderwood, of Addiewell Chemical Works, a quantity of a pure solid paraffin of low melting point. The plate electrodes were separated to a distance of $\frac{1}{3}$ inch inside a glass vessel, the liquefied paraffin poured in so as to cover the plates completely, and then allowed to solidify for twenty-four hours. When the plate electrodes were charged the first spark which passed was large and illuminated the whole of the paraffin; but the succeeding discharges were much smaller, and of equal amount. The first spark produced a deflection 3·6 times as great as the succeeding sparks. When the plate of paraffin was examined afterwards, it was found to be perforated in a zigzag manner, the hole being surrounded by char. We found that—

Electric Strengths.

Air, ...	1
Paraffin when solid, ...	5
Paraffin when liquid, . . .	2·5

Thus the electric strength of this substance, when in the solid state, is to its electric strength when in the liquid state as 2 to 1.



As an instance of how these experiments may be made directly useful, I may mention that we obtained two samples of liquid paraffin from Mr Calderwood to compare their electric strength. We found the ratio to be 1·6. It is, however, extremely difficult to effect the comparison unless we have a considerable quantity of each specimen. It is best to have a dish of the above form, where we can

have broad plates, the lower one slightly raised above the glass bottom, and the upper one well immersed in the liquid. The former of these conditions helps to prevent solid particles getting in between the plates, and the latter prevents the rising of the liquid up the stem, and consequent splashing about of liquid particles.

Discharge through Paraffin Vapour.—We put the discharging vessel, with a quantity of one of the pure liquid paraffins, inside the receiver of an air-pump, exhausted the air, and allowed the paraffin vapour to accumulate. When the barometer-gauge indicated 50 mm. pressure, the distance being $\frac{1}{3}$ inch, we took sparks through the vapour. The spark was of a broad section, green at either end, but of a deep violet between. When a quantity of air was let in, white jagged sparks were observed in the midst of the coloured spark. From the readings obtained at 50 mm. pressure, we infer that this paraffin vapour is 1·7 times as strong as air.

5. Laboratory Note. By Professor Tait.

Last autumn I received from Mr Maclachlan of Lower Green, Mitcham, some specimens of india-rubber tape which had been for several years wound, under considerable tension, helically round copper wire. At ordinary temperatures, after being peeled off, the material shows no tendency to contract; but Mr Maclachlan found that in hot water it almost instantly resumes its original dimensions.

I have recently reproduced almost exactly the same results by stretching sheet india-rubber, slightly warmed, helically round glass tubes, and immersing it for a short time in a freezing mixture. Some of the specimens thus produced in a few minutes compared favourably in their after behaviour in hot water with those of Mr Maclachlan.

Even without the use of a freezing mixture the effect may be produced, though not so perfectly, by drawing out the heated india-rubber to the point at which its intensibility begins to diminish very rapidly. If it be held for a few seconds in that state of extension, it shows very little tendency to contract till it is immersed again in hot water. Then it is instantly reduced to one-fourth or one-fifth of its previous length, but remains permanently stretched to three or four times its original length. This operation may be

performed many times in succession on the same specimen with the same results.

Professor Clerk-Maxwell informs me that similar results are to be obtained with gutta-percha, drawn out when *cooled*, after being boiled in water.

The subject is especially interesting as an exaggerated example of the *Elastische Nachwirkung*, which has recently been discussed at great length by Boltzmann and others.

The following Gentlemen were duly elected Fellows of the Society :—

J. B. Brown Morrison, of Finderlie and Murie, Perthshire.
Andrew Wilson, Ph.D., 118 Gilmore Place.
James Lambert Bailey, Ardrossan.
Robert Cox, Gorgie, Murrayfield.
John Hislop, Sec. to the Dep. of Education, New Zealand.
James Cossar Ewart, M.D., 12 Alva Street.
George Wm. Balfour, M.D., 17 Walker Street.

Monday, 20th January 1879.

DAVID STEVENSON, Mem. In. C.E., Vice-President, in the Chair.

The following Communications were read :—

1. On the Action of Heat on the Salts of Trimethyl-sulphine. No. III. By Professor Crum Brown and J. Adrian Blaikie, B.Sc.

I. *Acetate of Trimethyl-Sulphine.*

The acetate is formed by treating the iodide of trimethyl-sulphine with acetate of silver. On leaving the strong solution over sulphuric acid *in vacuo* for three weeks no crystallisation took place. The syrup on being heated in a small retort gave off water, and, without solidifying, sulphide of methyl, mixed with acetate of methyl. On redistilling the two latter, they went over at a temperature between 45°

and 56° C. It was not possible to separate them by distillation, but on shaking the mixture with solution of chloride of mercury, the sulphide of methyl was removed, leaving a few drops of acetate of methyl, easily recognised by its fruity smell.

$$(CH_3)_3S—C_2H_3O_2 = (CH_3)_2S + (CH_3)—C_2H_3O_2 .$$

II. *Benzoate of Trimethyl-sulphine.*

The benzoate is formed by treating the iodide of trimethyl-sulphine with benzoate of silver. The solution of the salt can be evaporated on the water bath to a syrup. On leaving it for about two weeks over sulphuric acid a very few crystals were formed, which could not be separated from the very thick syrup in which they were suspended. By adding alcohol it was obtained more easily in small thin plates. After several days of very cold weather a crust formed over the surface of the aqueous solution.

The thick aqueous solution on being heated to 100° C. with a current of dry air passing over it gave off some water, but the salt did not solidify. On continuing to heat at about 110° C., the clear liquid became milky, sulphide of methyl was given off, and a layer of a liquid formed above the heavy aqueous solution. This was collected apart, dried by means of chloride of calcium, and gave as its boiling point 198° C., that of benzoate of methyl. The decomposition is expressed by the following equation :—

$$(CH_3)_3S—C_7H_5O_2 = (CH_3)_2S + (CH_3)—C_7H_5O_2 .$$

III. *Dithionate of Trimethyl-sulphine.*

The dithionate is formed by neutralising an aqueous solution of free dithionic acid with the hydrate of trimethyl-sulphine. On evaporating a solution of the salt on the water bath it begins to crystallise out. On leaving the saturated solution to cool, a large quantity of clear cubical crystals was obtained, not hygroscopic, insoluble in hot alcohol, and, when dry, without any smell of sulphide of methyl.

Analysis agrees with the formula

$$\{(CH_3)_3S\}_2S_2O_6,\ H_2O.$$

On heating the salt to about 120° C., it loses water, and on raising the temperature to 220° C. sulphurous acid is given off, but at first no sulphide of methyl. After some time, sulphide of methyl begins to come off also, and the substance melts. Heat was applied until the melted substance, which had been perfectly clear, turned slightly brown, and the evolution of gas almost ceased. 8·015 grammes were found to have lost 3·325 grammes = 41·4 per cent. The loss of one molecule of water, one of sulphurous acid, and one of sulphide of methyl, corresponds to 43·3 per cent.

On cooling, the liquid solidified. The crystalline mass was very hygroscopic, and dissolved in alcohol. On adding ether, the salt was precipitated as a strong aqueous solution, which, on standing over sulphuric acid, yielded beautiful long fine prismatic needles. These were separated as well as possible from the mother liquid, by pressing between filter paper, and left for several days over sulphuric acid.

Analysis agrees with the formula

$$\left.\begin{matrix}(CH_3)_3S\\ CH_3\end{matrix}\right\}SO_4$$

and an examination of its properties proved it to be the methyl sulphate of trimethyl-sulphine.

$$(CH_3)_3S—O—SO_2—SO_2—O—(CH_3)_3S =$$
$$(CH_3)_2S + SO_2 + (CH_3)—O—SO_2—O—(CH_3)_3S\ .$$

2. Experimental Determination of the E. M. F. of the Gramme Magneto-Electric Machine at different Speeds. By Professor Tait.

3. On the Law of Cooling of Bars. By Professor Tait.

[Part of this paper appears in the *Transactions* for 1877–78, having been inserted (as § 11*) in Professor Tait's paper on *Thermal and Electric Conductivity*.]

4. Note on the Distribution of Temperature under the Ice in Linlithgow Loch. By J. Y. Buchanan, M.A.

The following observations of the temperature of the water at different depths below the ice covering Linlithgow Loch were made with one of Messrs Negretti & Zambra's "half-turn" self-registering thermometers, which proved to be a useful instrument for this species of inquiry. It was necessary, however, to fit it with a suitable inverting contrivance, as this part of the apparatus supplied by the makers is quite useless. The temperatures have received a provisional correction for error of graduation, and they may still have to receive a further, though certainly very slight, rectification, when a thorough comparison with a Kew standard has been made. The results are given in the table, to which are appended particulars of position and date corresponding to the stations.

Depth, Feet.		Temperature, Fahrenheit, at Station			
		No. 1.	No. 2.	No. 3.	No. 4.
	3	34·90	...	...	35·90
	6	35·25	36·10	36·00	36·30
	12	37·15	36·80	36·85	36·80
Bottom, . .	16	...	...	37·40	
Mud, . . .	16	...	...	37·80	
Bottom, . .	16½	38·50			
	18	...	36·95	...	36·90
	24	...	37·30	...	37·30
	30	...	37·40	...	37·40
	36	...	37·60	...	37·70
	42	...	...	...	38·40
	44	...	38·60		
Mud, . . .	46	...	...	...	39·85
Mud, . . .	47	...	39·75		

Particulars of Stations.—No. 1. Position approximately 70 or 80 yards from the steep bank below the Palace, which bears about S.S.E.* No actual bearings were taken, as my object was to test the action of the thermometer.—Date 6th January 1879.

* The bearings given are magnetic, and were taken with a pocket azimuth compass.

No. 2. Flagstaff on top of Town-hall bore . N. 160° E.
N.W. gable of Palace . . N. $125\frac{1}{2}$° E.
Date 9th January 1879.

No. 3. Flagstaff on Town-hall bore . . N. 154° E.
Centre of Rickles Island . . N. 63° E.
Date 9th January 1879.

The "bottom" temperature is that of the water a few inches from the mud. The "mud" temperature is that indicated by the thermometer when resting in the mud.

On this day, although a piercingly cold wind was blowing, the surface of the ice was thawing, and its structure could be observed. At Station No. 2, it was rendered quite opaque by air-bells, while at No. 3, these were present in much smaller quantity.

No. 4. Flagstaff on Town-hall bore . . N. 147° E.
Centre of Rickles Island . . N. 64° E.
Date 11th January 1879.

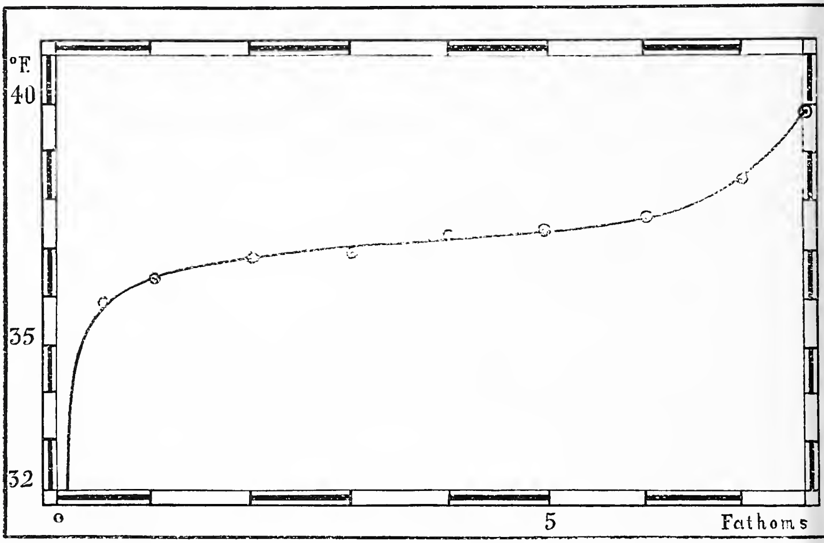
The ice at the surface was found to be $8\frac{1}{2}$ inches thick, and it was covered with a layer of freshly fallen snow 2 inches thick. The air was crisp and frosty.

No. 5. Flagstaff on Town-hall bore . . N. 152° E.
Centre of Rickles Island . . N. $65\frac{1}{2}$° E.
Date 20th January 1879.

Here a sample of the water was collected from a depth of 10 feet below the ice. No temperature observations were made.

The observations on the 6th January 1879, at Station No. 1 were made principally with a view to test the action of the thermometer and the inverting apparatus, and also to determine at what depth the water would be found at the temperature corresponding to its maximum density. The result of this day's operations was to show that the thermometer was suitable for such work, and to indicate two remarkable conditions of the water of the loch; namely

first, that the temperature above alluded to, 39·2° Fahr., was not observed at all at any depth between the ice and the bottom; and second, that the curve representing the vertical distribution of temperature had a point of contrary flexure, showing that the actual distribution could never have resulted from the freezing of a thin layer on the surface of a large volume of water at a uniform temperature, and the further cooling of it by conduction from the lower ice surface. The observed temperatures showed that, if the water had ever been at a uniform temperature throughout its depth, that temperature was certainly much below 39·2°, and that while the



water was being cooled by conduction downwards from the ice, it was being warmed by conduction and convection upwards from a source of heat at the bottom.

These unexpected results induced me to repeat my visits to the loch, and the observations at Stations Nos. 2, 3, and 4 were made on the 9th and 11th January. It will be seen that the results of them fully bear out the conclusions derived from the preliminary observations at Station No. 1. At Stations Nos. 2 and 4, situated in the deep western basin of the loch, the suspected heating surface is separated by a sufficiently thick stratum of water to enable its action to be studied separately. The observations at No. 4 are

represented graphically by the curve. From it we see that the temperature rises very quickly in the first fathom, then very slowly for some distance, until, in the neighbourhood of the bottom, it again rises quickly, reaching 39·85° in the mud. Had the water of the loch been in the condition usually imagined to immediately precede freezing—that is, at the temperature at which its water attains a maximum density uniformly throughout its depth, we should expect to find in the distribution of temperature in the water after the formation of ice the *remains* of this condition. The condition referred to would be represented graphically by a straight line drawn parallel to the line of depths through the temperature of maximum density, and if there were no supply or removal of heat from any other quarter than the surface, the curve of temperatures at any subsequent time when the loch was covered with ice would tend to coincide with this line at a sufficient depth. The source of heat which these observations show to exist at the bottom of Linlithgow Loch would have a tendency to mask but not to obliterate these remains. If the curve of Station No. 4 be studied in this light we discover the remains of a comparatively uniform temperature of approximately 37°, more than half the water being at a temperature between 36·5° and 37·5°. If we imagine the water to have been at a certain date uniformly at the temperature 37°, and the surface to have been suddenly covered with ice, and at the same time a source of heat to have been applied at the bottom, the distribution of temperature shortly afterwards would, I think, be of the kind represented in the curve. It must be observed that, assuming the water to be pure, both the loss of heat from the surface and the supply of it from the bottom would affect the intermediate waters by conduction alone until the temperature of the bottom had been raised to 39·2°. In the present case this temperature has just been passed, and, admitting the water to be pure, convection would be beginning to come into play. That it has begun to do so is shown by the flatness of the curve near the bottom, compared with its steepness near the ice.

Having established the existence of this unexpected thermal state of the water, it was necessary to find an explanation. The first that occurred to me was to suppose that the water of Linlithgow Loch was not pure water, but contained dissolved ingredients

sufficient to bring its temperature of maximum density down from 39·2° to about 37°. Three or four parts of common salt dissolved in one thousand parts of the water would be sufficient to produce this effect, and it did not seem extravagant to look upon this as the probable solution of the problem, especially as the loch is the receptacle for the whole sewage of the town of Linlithgow and the refuse of several works. Accordingly, on the 20th January, a sample of water was drawn from a depth of ten feet below the ice at Station No. 5, and examined. My supposition appeared likely to be confirmed by the overpoweringly horrible stench emitted by the water, which argued an amount of pollution not inconsistent with a large quantity of dissolved saline matter. On examination, however, it was found to be otherwise.

When freshly drawn the water was clear, but had become slightly turbid by the time it reached the laboratory; this turbidity disappeared on diluting the water with a large quantity of alcohol. It reacted neutral to test-papers. Its specific gravity was 1·00035 at 39·9° F., that of distilled water, at the same temperature, being unity.

The smell of the water was removed by boiling. Neither sulphate of copper nor alkaline acetate of lead solutions produced any precipitate of sulphides.

Finally, the water contained only 0·03 grammes chlorine in a litre, and was evidently remarkably free from saline ingredients.

It remains to determine by experiment the actual temperature at which this water attains a maximum density, and also, by observations in other lochs, whether a similar distribution of temperature is to be found.

Considering the excessively foul state of the loch, and especially of the mud at the bottom of the western part, the evolution of heat cannot be wondered at.

It will be seen from the table that the observations made at No. 2 on the 9th agree substantially with those made at No. 4 on the 11th January. At Stations Nos. 1 and 3 the depth of the water is nearly identical, but the temperatures at the same depths are different. At No. 1 the water near the surface is colder, and that near the bottom warmer than at No. 2. At No. 1 the temperature of the mud was not observed, but it would no doubt be

high, as that of the water a few inches above the bottom was 38·5°; the same water at No. 3 being 37·4° and the mud 37·8°. No. 1, being on the slope next the town, is exposed to the filth to be derived from it; it is therefore not surprising that the bottom water is warmer than at No. 3, which is situated on the banks near the northern shore, where the water is comparatively clear.

On the Principles of the Logical Algebra; with Applications. Part II. By Alexander Macfarlane, M.A., D.Sc.

(Read 20th January 1879.)

(*Abstract.*)

The equation

$$x^2 = x$$

expresses the condition that the symbol x be single and positive. The equation

$$x^2 = -x$$

expresses the condition that the symbol x be single and negative. The principle of contradiction

$$x(1-x) = 0$$

can be legitimately deduced with the help of the fundamental axioms of the science.

The memoir contains a general investigation of the conclusions which can be drawn from two or more premises:—

(1) of the form

$$x = m;$$

(2) of the form

$$xy = m;$$

(3) of the form

$$x = y + z;$$

and investigates the fundamental relations which subsist between single functions of any number of independent symbols.

Monday, 3d February 1879.

DAVID MILNE HOME, LL.D., Vice-President, in the Chair.

The Keith Prize for the biennial period 1875–77, which has been awarded to Professor HEDDLE, for his papers on the "Rhombohedral Carbonates," and on the "Felspars of Scotland," originally communicated to the Society, and containing important discoveries, was presented by the President (Professor Kelland) with the following remarks:—

PROFESSOR HEDDLE,—I am here to-night to exemplify a remark which is often made, that to insure success in an address such as I am about to deliver, the best way is to commit the charge of it to one absolutely ignorant of the subject. No false pride will then stand in the way of the best sources of information, nor will any undue admixture of half knowledge clog and darken the truth. For every particular contained in these remarks, then, I at once unhesitatingly acknowledge myself indebted to Professor Geikie. When I first became acquainted with this Society forty years ago, there used to frequent our meetings men who had the reputation of being mineralogists rather than geologists—Lord Greenock, Allan, and probably Jameson himself. That race has now died out, and with them mineralogy as a distinct science has all but lain dormant amongst us. During the preceding quarter of a century that science had flourished nowhere more vigorously than in Edinburgh. Professor Jameson introduced the definiteness and system of the Freyburg school, and infused into his pupils such a love of minerals that numerous private cabinets were formed; while under his fostering care the University Museum grew into a large and admirable series. One of my first acts as Professor in the University was to vote out of the Reid Fund, which had just come into our hands, a large sum (some thousands) to pay back monies expended on minerals throughout a series of years preceding. During those years geology, as the science is now understood, hardly existed. For as the nature and importance of the organic remains embedded in the rocks became recognised, their enormous value in the

elucidation of geological problems gradually drew observers away from the study of minerals. Consequently, as palæontology increased, mineralogy waned among us. To such an extent was the study of minerals neglected, that geologists even of high reputation could not distinguish many ordinary varieties. But as a knowledge of rocks presupposes an acquaintance more or less extensive with minerals, the neglect of mineralogy reacted most disadvantageously on that domain of geology which deals with the composition and structure of rocks. The nomenclature of the rocks of Britain sank into a state of confusion from which it is now only beginning to recover. To you, Professor Heddle, belongs the merit of having almost alone upheld the mineralogical reputation of your native country during these long years of depression. You have devoted your life to the study, and have made more analyses of minerals than any other observer. You have not contented yourself with determining their composition and their names; you have gone into every parish in the more mountainous regions, have searched them out in their native localities, and by this means have studied their geological relations, heaping up evidence from which to reason regarding their origin and history. After thirty years of continuous work you have communicated the results of your labours to this Society. For the first two of these papers, "On the Rhombohedral Carbonates," and "On the Felspars," in which you have greatly extended our knowledge of pseudomorphic change among minerals, enunciating a law of the shrinkage so frequently resulting therefrom, the Society proposes now to express its gratitude to you. The value of your papers is undoubted. Through the kindness of Mr Milne Home I have been favoured with the sight of letters addressed to you by four eminent mineralogists—Dana, of America; Rammelsberg, of Berlin; Szabo, of Buda-Pesth; and King, of Queen's College, Galway. Szabo states that the notice of Professor Heddle's paper on the Feldspars which appeared in Groth's "Zeitschrift für Mineralogie," greatly interests him, and makes him desirous of placing himself in direct communication with the author. Dana says, "I have read your paper on the Feldspars, in the 'Transactions' of the Royal Society of Edinburgh, with great satisfaction. Your thorough method of work leads towards important results of great geological as well as mineralogical value." I have the satis-

faction, in name of the Council of this Society, of presenting you with the Keith Medal. It is hoped that this recognition of your labours will not be without encouragement to you in the arduous researches in which you are engaged.

The following Gentlemen have been recommended by the Council to fill the vacancies in Foreign Honorary Fellowships caused by the deaths of Claude Bernard, Elias Magnus Fries, Henri Victor Regnault, Angelo Secchi:—

Frank Cornelius Donders, Utrecht.
Asa Gray, Cambridge, U.S.
Jules Janssen, Paris.
Johann Benedict Listing, Gottingen.

The following Communications were read:—

1. Chapters on the Mineralogy of Scotland. By Professor Heddle. Chapter V.

(*Abstract.*)

In this chapter Dr Heddle considered *the micas* occurring in Scotland. These he found to be Muscovite or Muscovy glass, and margarodite—of the white micas; Biotite, lepidomelane, and a new species which he proposes to call Haughtonite, after Professor Haughton of Dublin—of the dark micas. He also finds the species pihlite, hitherto unrecognised in Britain, occurring in quantity in the central districts of Banffshire, and in the west of Aberdeenshire.

In connection with the first of these micas, the mode of formation of exfiltration veins, and the metamorphism of gneiss into the grey granite of Aberdeenshire, were considered.

Dr Heddle found that margarodite is the glistening constituent of all the so-called talc slates which he had analysed; he doubts the occurrence of the last-named rock in Scotland, as no one of the rocks which he has examined, and which have passed by that name, was found to contain any talc.

Biotite in Scotland is characteristic of granular limestones, which also rarely contain margarodite. Lepidomelane, the ordinary dark-granite mica of Ireland, he had found in Scotland in only two localities.

Haughtonite, which is a ferrous oxide mica, with little magnesia, he finds to be the mica special to most of the grey and pale-coloured granites of Scotland; and it also occurs in the diorites of Banffshire. This new mica likewise occurs somewhat rarely on the Continent, though hitherto unrecognised as a distinct species.

2. On the Carboniferous Volcanic Rocks in the Basin of the Firth of Forth—their Structure in the Field and under the Microscope. By Professor Geikie, F.R.S.

(Abstract.)

In the introductory portion of the paper a sketch is given of the present state of opinion among Continental petrographers regarding volcanic rocks associated with palæozoic formations. The author points out that a relic of Werner's belief in the recentness of volcanic action may still be traced pervading the ideas of German geologists. In this paper he endeavours to show that, alike in their formation in the field and in their structure under the microscope, the basalts and tuffs of palæozoic time were as truly the products of volcanic action as the lavas and ashes of the present day. The area selected for description is the basin of the Firth of Forth. After an outline of the labours of previous observers in this classic region, the author sketches the history of volcanic action there, showing that volcanoes abounded and threw out an enormous pile of material during the time of the Lower Old Red Sandstone. After a long period of quiescence, the subterranean disturbances were renewed in an early part of the Carboniferous period, and continued until near the close of the deposition of the Carboniferous Limestone series. It is with the history and the products of this second volcanic epoch that the present communication deals.

The paper is divided into two parts. The first of these treats of the history of volcanic action during the Carboniferous period in the basin of the Firth of Forth. That area consists of six volcanic districts, in several cases remarkably independent of each other; and, though separated only by a few miles, yet producing very distinct forms of lava and tuff. The structure of the volcanic rocks in the field is then traced. Detailed descriptions are given

of some of the more typical necks or volcanic funnels which supplied the sheets of rock now so abundant at the surface, and comparisons are drawn between their characters and those of tertiary and recent cones and craters. The numerous intrusive sheets and dykes are described in their relation to the surrounding rocks and to the position of the volcanic vents. An account is given of the bedded lavas and tuffs so copiously interstratified with the Carboniferous formations, and their identity with modern volcanic masses is insisted on.

The second part of the paper deals with the petrography of the igneous rocks, and more especially with their characters as revealed by the microscope. The author states that he has been engaged during the last ten years in carrying on this investigation, and that he has studied some hundreds of slices of the rocks from all parts of the region. After alluding to what has been published by other observers on this subject, he proceeds to describe the rocks under the two main divisions of, I. Crystalline; II. Fragmental.

I. Crystalline.—These embrace all the melted or lavaform rocks. They may be subdivided into four classes:—1st, *Augite-Felspar Rocks,* which include three types of structure—(1) granitoid, consisting of a crystalline mixture of a triclinic felspar (but sometimes orthoclase), augite, titaniferous iron, and apatite, with occasionally biotite, and more rarely quartz; (2) Doleritic—a crystalline mixture of triclinic felspar and augite, with titaniferous iron or magnetite, apatite, and frequently olivine, with a variable proportion of a half-glassy ground-mass; (3) Basaltic—a mixture of minutely granulated (and larger, more definitely crystallised) augite, triclinic felspar, magnetite, and olivine, with usually some apatite in a glassy or half-glassy ground mass. 2d. *Olivine* (*Serpentine*) *Augite Rocks,* consisting mainly of serpentine throughout, while abundant crystals of altered olivine occur, fresh augite, titaniferous or magnetic iron, apatite, and occasionally traces of a triclinic felspar. Where the last-named mineral increases in amount, the rock assumes the character of a much altered "diabase." 3d. *Felspar-magnetite Rocks,* consisting essentially of a triclinic felspar and grains or shred-like particles of a black magnetic mineral, sometimes with large orthoclase crystals, rarely with augite, in a porphyry ground-mass. 4th. *Orthoclase Rocks* or *Felsites.*

II. Fragmental.—The rocks included in this class embrace all the agglomerates, breccias, and tuffs. Their characters are described in detail, and it is shown that they consist mainly of the granular *peperino* or detritus of the lavas.

3. Exhibition of Specimens of Auriferous Quartz from the Leadhills District By Patrick Dudgeon, Esq. of Cargen.

(*Abstract.*)

Mr Dudgeon exhibited some specimens of auriferous quartz from Wanlockhead and Leadhills districts.

No. 1 was found in July 1878, by Thomas Tennant in Wingate burn, Leadhills; it is a very rich specimen, and is now placed in the collection of Scotch minerals in the Museum of Science and Art.

No. 2 was found by Eleanor Handcock in April 1878, on the banks of the Wanlock water, Duntercleuch.

No. 3, found by E. Benzie, daughter of police constable at Leadhills, in Glengonar water, Leadhills, in 1878.

No. 4, found in Longcleuch Burn, Leadhills, and in the possession of Mr Joseph Gill, local factor to the Earl of Hopetoun.

Declarations as to No. 1 and 2 made before a Justice of Peace, as to the place and date they were found, accompanied the specimens.

The following Gentlemen were duly elected Fellows of the Society:—

Major-General A. Cuningham Robertson, C.B., 86 Great King Street.
William Denny, Bellfield, Dumbarton.
Dr Francis W. Moinet, F.R.C.P.E., 13 Alva Street.

Monday, 17th February 1879.

Professor KELLAND, President, in the Chair.

The following Communications were read:—

1. On some Physiological Results of Temperature Variation. By J. B. Haycraft, M.B., C.M. Communicated by Professor Turner.

2. On the Elasticity of the Walls of the Arteries and Veins. By Dr Roy. Communicated by Dr George W. Balfour.

3. Further Note on the Distribution of Temperature under the Ice in Linlithgow Loch. By Mr J. Y. Buchanan.

In continuation of the observations into the condition of the water of frozen lochs, which were communicated to the Society at the meeting of the 20th January 1879, I have been able to repeat the observations in Linlithgow Loch on two separate days, and also to visit Loch Lomond. The observations in Linlithgow Loch were made on the 25th January and 1st February, both times at the same spot, from which the Court House Flagstaff bore N. 150½° E., and the Rickles Island N. 63½° E., the depth being 48 feet. On the 25th January there was a diminution of pressure under the ice, so that when it was pierced the air rushed in with a roaring noise for about a minute, when it stopped, and the water rose in the hole. On the 1st February, on the other hand, the ice was cracking and resounding on all sides, and water rose at once in the hole when the ice was pierced, there being at the same time a considerable escape of air. These two stations have been numbered respectively 6 and 7; they are exactly in the same spot, a few yards distant from that of No. 4. The ice was very decidedly thicker than it had been.

Depth in Feet.	Temperature Fahr. at Station	
	No. 6.	No. 7.
3	36·00	36·00
6	36·60	36·80
12	37·35	37·50
18	37·35	37·80
21	...	37·80
24	37·50	38·15
30	37·90	38·30
36	38·45	39·00
42	39·80	40·70
45	...	42·00
Mud, 48	41·70	$\begin{cases} 42{\cdot}00 \\ 42{\cdot}05 \end{cases}$

From these observations we have the mean temperature of the 48 feet of water 37·83° on 25th January, and 38·28° on the 1st February. The heat required to produce this rise of temperature is very considerable, and would require the combustion of very nearly two tons of coal per acre.

It was shown that the mineral constituents of this water were insufficient to produce a lowering of its temperature of maximum density; it was, however, uncertain whether this effect might not have been produced by the substances which gave the water its peculiar odour. The actual temperature of maximum density of the water was accordingly compared with that of distilled water in the same piezometer, and no difference could be detected.

Hence it was evident that, before being completely frozen over, Linlithgow Loch must have been cooled down throughout to a mean temperature of certainly two degrees lower than that of its maximum density. As this conclusion was at variance with the generally accepted doctrine, it was necessary to test it by observations in another lake, and in one of undoubted purity. The result of observations made with this purpose in Loch Lomond on the 28th and 29th January was that the general lowering of temperature was even greater than in Linlithgow Loch, the mean temperature under the ice at a spot 51 feet deep being 34·05° F. Observations under the ice were made at four stations between Balloch and Luss, and several observations were made in the water off the edge of the ice,

where it terminated towards the upper and deeper reaches of the loch.

It results, from the discussion of these results, that the phenomena attending the freezing of a fresh-water lake may be stated as follows :—

First. Cooling down of the whole water to an approximately uniform temperature of 39·2° F.

Second. Local formation of ice generally as a shore fringe.

Third. Convection currents flowing at the surface from the ice to the middle of the loch, and at the bottom from the middle to the edge, caused by the disturbance of the equilibrium in consequence of the local occurrence of ice.

Fourth. Continuance of these currents for some time without appreciable diminution of strength, then gradual slackening of these currents as the temperature of the open water becomes lower, and the consequent difference of density on which their existence depends becomes less.

Fifth. The velocity of the water leaving the ice fringe and the facilities for losing heat at the surface, by radiation or convection, are so related that a portion of water leaving the ice fringe freezes before it can mix with the warmer water off shore.

Sixth. When condition fifth has been attained, rapid propagation of ice over the surface, and complete covering of the water with ice.

Seventh. Existence of the water under the exceptional condition of a uniform surface temperature, and consequent rapid equalising of differences of density in different parts. Did the loch consist of a mass of water enclosed in a basin which neither supplied nor removed heat, the condition of the water would alter only very slowly, owing to variation in thickness of the ice covering, and the gradual alteration of temperature of the water by conduction from the lower ice surface.

Hence the temperature of the bulk of the water under the ice of a frozen lake will tend to be uniform, and the uniform temperature to which it approximates will be determined by a number of local circumstances. It will lie between 39·2° and 32° F., and will depend on the shape and position of the loch as well as the geographical features of the surrounding land, and especially on the severity of the weather. The body of a loch will be cooled more

when it has been frozen by a moderate and comparatively long-continued frost than when the ice has been formed quickly by very severe frost. For the more severe the frost the sooner will it be able to overtake the water leaving the ice fringe; in other words, the stronger will be the current which it will be able to arrest, the greater the *head* which it will be able to stem. But the head is caused by the higher temperature of the open water as compared with that under the ice. Hence the more severe the frost, the higher will be the temperature which it will be able to fix.

Eighth. Condition seventh is affected by heat supplied from the bottom. The amount due to the internal heat of the earth is certainly under present circumstances inappreciable. That due to decay of organic material in the water or at the bottom is also in all probability insensible in lochs of such purity as Loch Lomond, though it is of very serious importance in polluted lakes like Linlithgow.

Ninth. The change of the water of a lake affects the distribution of temperature after it is frozen. When the loch is entirely frozen over, the supply is delivered entirely at the surface, and therefore sensibly at a temperature of 32° F., which is also the temperature of the outflow. This water finds its way from its source to the outlet close under the ice, and lowers slightly the temperature of the water in the neighbourhood of the ice. The water which finds its way from the open part to the outlet does so along the bottom, and it is taken from the deep warm water of the open part in virtue of the convection currents at the edge of the ice. The temperature of the supply thus furnished to the frozen basin depends first on that of its source, namely, the deep water of the open part of the lake, and then on the configuration of the bottom, more especially on the maximum depth on the ridge separating the frozen basin from the open one. The shallower the water on the ridge the nearer will the deep water have to come to the surface in order to surmount it, and consequently the colder will it become.

The rising of warm water to the surface was very manifest at the passage between Inchtavannach and the Dumbartonshire shore of the loch.

Monday, 3d March 1879.

Professor MACLAGAN, Vice-President, in the Chair.

The following Communications were read:—

1. Heating and Ventilating of Churches and other Buildings: Report of a Series of Experiments made in a Hall in Upper Grove Place—Dimensions 50×25 feet, height about 20 feet; a Stove Chamber being outside at the south end, its roof about 8 feet in height; the only direct connection with the Hall being an opening in the mutual wall about 7 or 8 feet from the floor, and between 3 and 4 square feet dimensions. By Charles J. Henderson, Edinburgh. Communicated by Professor Jenkin.

Thermometers were placed throughout the hall, one on each of the four walls, 6 feet from the floor, the one on the south wall being a couple of feet below the inlet for the hot air. Three were placed along the centre of the roof, 2 feet or so under the roof. In the stove-chamber, first one and afterwards two stoves were placed, with double smoke-pipes before entering the chimney.

The trials were made for many weeks, and the tables on the two following pages give a fair representation of the results obtained in the warming of the hall; the effect on the four thermometers, 6 feet from the floor on all the four walls, being so remarkable as to induce my submitting these experiments to the notice of the Royal Society. Precisely as the heat, accumulated in the stove-room, entered the hall, each of these four thermometers rose in an equal degree, as shown in the tables, though the one at the north end was 50 feet off. The effect on the three roof thermometers is also shown in Table No. II. The stove-room had a doorway to outside, which during heating was only opened to a small extent.

It is worthy of notice the amount of heat obtained from the small quantity of coals consumed, which it is thought is the result of the peculiar construction of the stove smoke-pipes, each having four flanges; and the effect of placing two stoves in the chamber gives a very remarkable result as regards the fuel consumed, it being much the same as for one; showing the advantage, where much heat is required, of increasing the number and not the size of the stoves.

TABLE No. I.

Hall in Upper Grove Place, 50 × 25 Feet.

Time.	Outside.	Opening into Hall.	Six Feet from Floor.			
			North Wall.	South Wall.	East Wall.	West Wall.
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.
7 A.M. .	33	42	43	42	42	42
7.30 „ .	33	100	43	43	43	43
8 „ .	33	138	45	45	44	44
8.30 „ .	33	152	48	47	47	47
9 „ .	34	180	53	52	50	51
9.30 „ .	34	186	57	55	54	55
10 „ .	34	208	60	59	57	58
10.30 „ .	34	212	64	64	61	62
11 „ .	35	218	66	65	65	64
*11.30 „ .	35	224	66	67	66	66
12 „ .	36	228	67	68	67	67
	Ventilator in Hall opened and Stove-Room Door raised.					
12.30 P.M. .	36	142	62	63	63	62
1 „ .	37	136	60	61	61	60
1.30 „ .	37	110	59	61	60	59
2 „ .	37	104	59	60	59	59
2.30 „ .	38	90	58	60	59	59
3 „ .	38	84	58	60	58	58
3.30 „ .	38	76	58	60	58	58
4 „ .	38	70	57	59	57	57

Two Stoves, with Double Pipes. $\frac{3}{4}$ cwt. Coals used.

Time.	Outside.	Opening into Hall.	Six feet from Floor.			
			North Wall.	South Wall.	East Wall.	West Wall.
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.
6 A.M. .	33	42	42	42	42	42
6.30 „ .	33	76	42	42	42	43
7 „ .	33	90	43	43	43	43
7.30 „ .	34	100	44	45	44	44
8 „ .	34	112	45	46	45	45
8.30 „ .	34	132	47	48	46	47
9 „ .	35	138	50	51	49	49
9.30 „ .	35	144	53	53	51	51
10 „ .	35	146	54	55	52	52
10.30 „ .	35	148	55	56	54	53
11 „ .	35	149	56	57	55	54
11.30 „ .	35	154	56	57	56	55
12 „ .	36	156	57	58	57	57

Single Stove, with Double Pipes. $\frac{3}{4}$ cwt. Coals used.

* No more coals put on.

Table No. II.

Time.	Thermometer Outside.	Thermometer on Wall, 6 feet from floor.				South opening in Wall. Hot air enters.	South end of Ceiling.	Centre of Ceiling.	North end of Ceiling.	Front of Plat-form.
		North.	South.	East.	West.					
A.M.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.
6	44	52	52	52	53	53	53	53	52	52
6.30	45	53	53	52	53	102	76	59	59	56
7	45	56	56	54	54	147	96	72	70	62
7.30	45	60	60	59	59	182	126	90	86	71
8	46	65	65	62	62	202	136	97	93	77
8.30	47	69	69	67	68	228	164	110	106	85
* 9	48	71	72	70	70	230	166	118	112	89
9.30	*48*	*76*	*78*	*75*	*75*	*181*	*142*	*111*	*108*	*94*
10	*48*	*76*	*78*	*75*	*75*	*157*	*132*	*105*	*102*	*92*
10.30	*49*	*75*	*77*	*74*	*74*	*139*	*118*	*98*	*96*	*89*
11	*50*	*73*	*76*	*72*	*72*	*122*	*104*	*90*	*89*	*85*
11.30	*50*	*71*	*74*	*70*	*70*	*107*	*93*	*84*	*83*	*80*
12	*51*	*70*	*72*	*69*	*69*	*97*	*86*	*79*	*78*	*77*
P.M.										
12.30	*52*	*69*	*72*	*68*	*68*	*93*	*82*	*77*	*76*	*77*
1	*52*	*69*	*71*	*68*	*66*	*90*	*80*	*76*	*75*	*75*
1.30	*52*	*69*	*71*	*68*	*68*	*84*	*77*	*75*	*74*	*74*
2	*53*	*69*	*71*	*68*	*68*	*80*	*74*	*73*	*73*	*74*

Two Stoves, with Double Smoke-pipes, lighted at 6 A.M. Fire taken off 9 A.M. Hall Ventilators opened 1.30 P.M. Coals consumed, 70 lbs.

Note.—The figures in italics show the amount of heat got from the large surface of heated iron *after the fuel has been exhausted.*

Time.	Thermometer Outside.	Thermometer on Wall, 6 feet from Floor.				South opening in Wall. Hot air enters.	South end of Ceiling.	Centre of Ceiling.	North end of Ceiling.	Front of Plat-form.
		North.	South.	East.	West.					
A.M.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.
7	43	50	50	50	50	51	50	50	50	50
7.30	44	52	52	51	52	84	66	58	56	55
8	44	54	54	52	53	110	80	64	62	58
8.30	45	56	56	55	55	132	89	73	71	62
9	45	57	57	57	57	142	95	79	77	65
9.30	46	61	61	59	59	144	102	82	80	69
10	46	62	62	60	61	144	102	82	80	71
10.30	48	64	63	62	62	148	106	85	83	73
11	49	65	64	63	63	146	104	85	83	74
11.30	50	65	65	63	63	144	103	85	83	74
12	50	66	66	64	64	145	103	86	84	75

Single Stove with Double Pipes. Coals burned, 70 lbs. Stove lighted at 7 A.M.

* Fire taken off.

Various suggestions have occurred to me since these experiments were made for further improving the system, which I may shortly mention.

1. As to the stove-room, it ought to be lined in the inner portion with some radiating metal or non-conducting substance, and be divided into two parts, one containing the bodies of the stoves or pipes, the other for firing, &c., a doorway being left between the two for the admission of such amount of air as required. And further, as the quick removal of the heat given off from the metal of the stoves or pipes is of importance, it is proposed to introduce a fan in the inner division to be worked from the outer, and to admit as small a portion of cold air as may be practicable for carrying off the heat into the hall or church.

From the result of these experiments I have come to the conclusion that for warming and ventilating buildings a stove-room is required, but the best mode of raising heat therein is an open question.

For large buildings I believe steam-pipes placed in the chamber would be the cheapest, the boiler being either in the chamber or outside. The heat would be greater than from hot water, but this latter would answer very well, and, perhaps, on the whole, would be preferable, the amount of heat being regulated by the extent of pipes; and it cannot fail to occur to every one that the result obtained from the above experiments of the effect of *accumulated* heat discharged into a building in the manner described would in all respects be preferable to the present system of pipes distributed in the building itself. It is a mistake to introduce heat into a church or hall by dispersing it in pipes covered with gratings; the waste of heat must be very great, while, if the same amount were *accumulated* in a chamber, and sent on, as described above, it will be distributed in a way so perfectly suitable to what is required, as to cause wonder that so valuable a property of heat had not long ago been known.

Ventilation.

The stove-chamber is itself an efficient means for the introduction of fresh moderately warm air, by simply opening the outer door and allowing the air to pass over the heated stoves or pipes, aided, if required, by using the fan. But the more important matter still

remains of getting quit of the vitiated air produced in crowded assemblies. This also formed the subject of experiment in the hall by my having temporary wooden tubes, 12×6 inches wide, placed along the walls about 6 feet from the floor, and discharging outside the roof. These, in a crowded meeting, were found to be very efficient in carrying off the vitiated air, though occasionally a back-draught came down; but were the discharge into a cavity above the ceiling, or an archimedean can at the opening, the back-draught would be avoided.

I have perfect faith in the efficiency of these wall-tubes in carrying off the vitiated air, and may mention an attempt I made for a visible proof of this. I burnt brown paper to produce smoke. The smoke ascended only a few feet, and then spread out horizontally in a cloud, and when near any of the tubes it was drawn up.

Remarks.

In Table No. II. the course of the hot air on entering the hall is pretty well shown. The larger portion, of course, mounts towards the roof, along which it travels at somewhat different temperatures. Not so, however, below where the audience sits,—there the gradually-rising temperature is practically equal in all corners of the hall, and that without any appreciable difference in time between the effects on the several thermometers.

2. Chapters on the Mineralogy of Scotland. By Professor Heddle. Chapter VI. "Chloritic Minerals."

In this Chapter, Dr Heddle discussed the substances usually thrown together, under the term of Chloritic Minerals. He showed, by an extensive series of analyses, that they were to be divided into three groups—those which occurred in metamorphic rocks, in recent strata, and in volcanic rocks.

He proposed to confine the term Chloritic to the minerals which are found in metamorphic rocks, and to apply the term, the Saponites, to those which occur in volcanic rocks.

In Scotland, metamorphic rocks afforded the species Pennina, Ripidolite, Chlorite, and Chloritoid. The New Red Sandstone of Elgin yielded Glauconite. Volcanic rocks contained, plugging up

their steam holes, Delessite, Chlorophæite, Hullite, Saponite, and Celadonite.

Of these, Delessite seemed to be confined to igneous rocks of Old Red Sandstone age—Chlorophæite and Hullite to more recent volcanics; while the others occurred in rocks of both of these ages.

He doubted whether the so-called *Viridite* of petrologists had any claim to a specific title—possibly it might be either Delessite, Saponite, or Celadonite. He regarded it as most probably the last of these. No attempt had been made to show that it was not an already named substance; and until there was good appearance of this, it was in no way entitled to a place in mineral nomenclature.

Two new minerals, belonging to the first of these groups, were noticed as occurring in granite near Tongue in Sutherland, and in Rubislaw quarry.

3. On Deep-Sea Thermometers. By Mr J. Y. Buchanan.

For the purpose of observing the temperature of the waters below the surface in lakes and seas, two classes of thermometers have been used—namely, ordinary thermometers and self-registering ones. The earliest observations were made with the ordinary thermometer, and it was used in one of two ways—either it was sunk itself to the desired depth, and was so enveloped and protected by badly conducting material, that in bringing it up again through the layers of water of different temperature it had not time to alter its own temperature, or a quantity of the water at the desired depth was enclosed in a bucket of suitable construction and brought to the surface, and then immediately tested with the thermometer. Many very excellent and trustworthy observations exist which have been made in one of these ways. Our first knowledge of the temperature of the deep water of fresh-water lakes was obtained from the observations of Saussure on the lakes of Switzerland, made with a thermometer so padded and protected that it could be drawn up through 1000 feet of water of any temperature likely to be found in nature without sensibly altering its temperature. The self-acting bucket or sea-gauge was used at an earlier date in the determination of the temperature of the deep water of the ocean. The accuracy of the results obtained by this method depends greatly on the skill of the observer. In the case of Saussure and of Fischer and Brunner, the

results are clearly to be relied on implicitly. In the experiments with the sea bucket, also, excellent results have been obtained. The results obtained by both methods of experimenting will be the more accurate the more uniform the temperature of the water. The temperature, especially of the bottom water, has also frequently been determined by bringing up a quantity of the mud, and taking its temperature when it arrives on board. This method also gives very satisfactory results when a considerable quantity of mud is at disposal.

Self-Registering Thermometers.—By far the greatest number of observations has been made with self-registering thermometers of one form or another.

The first self-registering thermometer was made by Cavendish.* He constructed both a maximum and a minimum thermometer, and they were of the kind called by the French *à deversement*, *out-flow* thermometers. In fact, his maximum thermometer is in every particular identical with that known in France as Walferdin's; his minimum is on the same principle, but has a U-formed stem instead of a straight one. The disadvantages of this form of thermometer are two—namely, the indications are not continuous, but by jerks, depending on the size of the mercury drops, and they require to be constantly set, the maximum at a higher and the minimum at a lower temperature than the one to be observed; they also require constant comparison with a standard. They are, therefore, not suitable for use where many observations have to be made expeditiously.

In the year 1782 Six† published a description of the combined maximum and minimum thermometer which bears his name, and which has since continued to assert its place among meteorological instruments as perhaps the best self-registering thermometer. The instrument is too well known to require particular description. It may, however, be noted that Six himself did not use a hair for a spring to keep his indices from falling down, but a fine glass thread soldered to the top of the index, and sticking up in a direction very slightly inclined to that of the length of the index, so that it pressed gently against the sides of the tube. The advantage of the glass

* Phil. Trans., 1758, l. p. 308.

† Phil. Trans., lxxii. p. 72.

over the hair is that it does not lose its elasticity; but, on the other hand, the index takes up more room, and requires a thermometer with a longer stem.

Maximum and minimum thermometers such as Cavendish's and Six's, when used for deep-sea exploration, show only the maximum and minimum temperatures to which they have been exposed in any one excursion, and a single observation with such a thermometer does not give us with certainty the temperature of the water at the depth to which it has been sunk. Hence, if we had a right to assume that the temperature of a sea or lake might vary in any conceivable way with the depth, these instruments would be valueless. We have, however, no right to make this assumption; we know, on the contrary, that in all seas whose surface is not exposed to a freezing temperature, the temperature of the water will as a rule diminish as the depth increases; that, therefore, the minimum temperature, as shown by the self-registering thermometer, will, in fact, be the temperature at the greatest depth attained by this thermometer. Hence, in such cases, this instrument is to be relied on, and more especially when *series* of temperatures are taken—that is, when the temperatures at different depths in the same locality are taken, so that the evidence of the decrease of temperature with increase of depth is rendered as strong as possible. In order to render an account of the state of any lake or sea as regards temperature, it is absolutely necessary to have such serial observations; hence, for such investigations, the maximum and minimum thermometer is not only perfectly trustworthy, but a most valuable and, indeed, indispensable instrument, for it has the great advantage that, as it is in the strictest sense *self*-registering, any number can be attached to the same line, and so at one haul the temperature can be observed at a number of different depths.

For isolated observations the thermometers just described are not so satisfactory, and a very great amount of ingenuity has been displayed in the invention of machines for registering the actual temperature of the water at any depth independently of that of the water above it. None of the instruments devised for this purpose have been strictly *self*-registering; they have all required some assistance from the observer, who, by various forms of mechanical appliance, brings about a catastrophe which leaves its mark on the

condition of the instrument. It is obvious that any control which an observer may have over an instrument separated from him by, it may be, three or four miles of cord, is very limited, and is, in fact, confined to his ability to move it towards or from him. By a simple mechanical contrivance this longitudinal motion may be made to produce one of rotation, and, in fact, the assistance afforded by the observer to the thermometer to enable it to register its own temperature consists in his turning it either upside down or through a whole circle when it has reached the desired depth. The first observer who made use of this device was Aimé. His working arrangement is described in Ann. Chim. Phys. 1843 [3] vii. p. 497. It is worked by a weight, which is allowed to slip down the line, and which then sets free the apparatus. His *thermomètre à bascule*, along with a number of ingenious modifications of existing forms, is described in the same journal, 1845 [3] xv. p. 5. It was unfortunately only after he was obliged to leave the Mediterranean, which had been the scene of his labours, that he invented the very elegant combination of thermometers by which he was enabled to ascertain the temperature at any depth, no matter what the intervening distribution might be. It is described in the memoir just cited. It consists of two outflow thermometers, so constructed that the one of them registers the sum of the rises of temperature, and the other the sum of the falls of temperature, to which it is exposed in any excursion. When they have reached the required depth they are inverted, and on their way back to the surface they register, as above described, the rises and falls of temperature to which they are exposed. If r be the sum of the rises of temperature, and f the sum of the falls, s the temperature of the surface, then the temperature at the depth where they were inverted will be $d=s+r-f$.

If they are allowed to register on the way down, and then inverted at the greatest depth, so as not to register on the way up, the effect will be precisely the same, though the functions of the thermometers will be reversed.

Beautiful and ingenious as Aimé's thermometers are, they have the disadvantages common to all outflow thermometers; they are neither simple enough nor handy enough for work involving many observations. The inverting thermometer, patented by Messrs

Negretti & Zambra, satisfies the conditions required of a thermometer for isolated observations as completely as can be hoped for. It is a mercurial thermometer; the bore of the stem is contracted to the smallest possible diameter at a point about an inch from the neck of the bulb. As long as the thermometer is standing vertically stem uppermost, the mercury is continuous in stem and bulb, but if it be inverted the mercury parts at the contraction, the portion in the stem falling down into the point. The stem is graduated from the point towards the bulb, and the temperature at the moment of inversion is read off by the height of the mercury in the end of the stem. This thermometer exists in two varieties, the one with a straight stem, which registers by simple inversion, the other with a U-formed stem, which requires to be turned completely round. The turning arrangement for the latter instrument is a somewhat elaborate and expensive instrument, but it answers its purpose. The inverting arrangement, supplied with the half-turn thermometer, is somewhat clumsy and unsatisfactory. The half-turn instrument, when fitted with a suitable inverting arrangement, is to be preferred to the others in all work at moderate depths. For ocean work it would probably be necessary to give up the protection of the whole stem, as it would be impossible to guarantee a tube, which can contain the whole instrument, against collapse when exposed to pressures of over 500 or 1000 fathoms of water. If the bulb and the twist on the stem were protected it would be quite sufficient.

Sources of Error in the Indications of various Thermometers.—When an ordinary thermometer, protected by badly-conducting envelopes, is used, it is obviously exposed to alteration of temperature by being pulled through warmer or colder water on its way to the surface. Whether any sensible error is likely to result from this cause must be determined in each particular case by experience. The more perfectly it resists change of temperature the longer it will take to assume the temperature of the water. Saussure left his thermometer down for twelve or fourteen hours for each observation, so that this method is now seldom used. Similarly, also, the method which depends on bringing up a sample of the water in a vessel fitted for the purpose, and taking its temperature with an ordinary thermometer when it reaches the surface, has been discontinued, for although it does not take much more time than would

be necessary for sending down a thermometer and bringing it up, it is impossible to bring up water from great depths in warm climates without sensible change of temperature.

In the case of outflow thermometers, the delicacy of the instrument is limited by the size of the mercury drops. In the inverting thermometers of Negretti and Zambra an error may arise from the difference of volume of the mercury in the stem at the temperature at which it was inverted, and at that at which it is read. In an extreme case this may amount to as much as 0·4° F.; it can, however, be allowed for.

In Six's instruments there is a possible error from looseness of the indices, in consequence of which they are apt to be shaken out of their places by any jarring of the line. Errors from this source can be avoided to a great extent by attaching the thermometer to the line by means of an elastic or india-rubber stop.

All the self-registering instruments are liable to error from the effects of pressure. The pressure inside a thermometer is never greater than that of the atmosphere when it was sealed up. It may, however, be exposed outside to a pressure of 500 or 600 atmospheres. The effect of this difference of pressure on the outside and inside of the glass envelope is to make it tend to collapse. The bulb of the thermometer is squeezed, and its volume in consequence diminished. The liquid which it contains is thereby forced into the stem, and its *apparent volume* is greater than it would have been had there been no excess of pressure on the outside of the instrument. The temperature of the instrument is measured by the apparent volume of the liquid which it contains; hence the effect of pressure is to raise the observed temperature above the true temperature. Parrot and Lenz,* in 1832, made a series of experiments on the effect of pressure on thermometers. They experimented at pressures up to 100 atmospheres, and observed differences between the apparent and the true temperatures of as much as 20° C. They found that for the same instrument the compression was simply proportional to the pressure. They used a thermometer as a manometer. After this date it was usual to attempt some kind of protection for self-registering thermometers. Those with straight stems, such as Walferdin's mini-

* Mem. del. Acad. Petersb. 6[e] Série ii. p. 264.

mum, were sealed up in glass tubes, and so completely protected. Those whose stems were bent had to be enclosed in metal cases closed with a screw. This form of protection never answered well, as it was impossible to screw on the cover so tight that water under the great pressures met with at considerable depths would not find its way in. In order not to have to abandon the use of thermometers of the convenient form of Six's, the device of protecting the bulb only was hit upon, and it appears that the first thermometer of this kind was used by Captain Pullen on board H.M.S. *Cyclops.* The effect of pressure on the stem is quite insignificant, and under ordinary circumstances insensible. For, in nearly all seas where the surface temperature is over 40° F., the temperature of the water diminishes as the depth increases, and therefore it is the *minimum* leg which is used, and the effective part of it is that filled with spirit, which may have a length of at most three inches. The effect of pressure in diminishing the volume of a short piece of thermometer tubing must certainly be very small, but its actual value can only be determined by removing the bulb and taking the piece of the stem between the mercury and the neck of the bulb as the bulb of a new thermometer, and determining experimentally the effect of pressure on it. An approximation to the effect may be made by exposing the thermometer to various high pressures at known temperatures and observing the rise of the maximum index, then removing the bulb and calibrating the stem. Knowing, then, the ratio of the volume of this part of the minimum leg filled with spirit to the whole volume, from the bulb to the maximum index, it may be assumed that the compression will be in the same ratio. And this value will probably be greater than the real one, for the compression of the water produces of itself a certain rise of temperature, and consequently raises the maximum index. This can, however, be estimated either by comparison with a completely protected thermometer, or by bringing the minimum index also home on the mercury before raising the pressure. If, then, there has been a rise of temperature caused by compression, there will be a corresponding lowering of temperature on relieving the pressure. If the compression apparatus be allowed to stand, after the pressure is up, until it has dissipated the heat evolved by the compression, the

relief of pressure will cause a corresponding absorption of heat which will show itself in the position of the minimum index. Some experiments which I have made in this direction show a lowering of temperature of 0·3° F. for the relief of a pressure of $2\frac{1}{4}$ tons per square inch, the whole rise of the maximum index having been 1·8° F.

We may, I think, be quite certain that when the minimum leg is the one used and the temperature low, the error caused by the effect of pressure on the stem is inappreciable.

Cavendish, who invented the self-registering thermometer, foresaw also the most important of the uses to which it could be applied. Thus he suggests that the higher regions of the atmosphere might be investigated by attaching it to a kite—balloons not having been invented. With regard to deep-sea explorations, he says: "If instruments of the nature above described were to be used for finding the temper of the sea at great depths, some alteration would be necessary in the construction of them, principally on account of the great pressure of the water, the ill effect of which can, I believe, be prevented no other way than by leaving the tube open."

This was written in 1757, and it was not till 1762 that Canton proved that liquids are compressible. Cavendish therefore hoped that as the pressure would not produce distortion of the glass when the tube was open, it would have no visible effect on the apparent volume of the liquid. The device of leaving his thermometer open at the end was adopted by Aimé in some of his experiments, the effect of pressure on the apparent volume of the liquid being determined independently, and a correction applied accordingly. I devised and constructed a mercurial thermometer, or *piezometer*, on the same principle,* but my object in admitting the water pressure to the inside of the instrument was to utilise it in shifting the scale of the thermometer as the depths changed. The thing registered in such instruments is always the *apparent* volume of the liquid, and this varies with the temperature and the pressure. Hence the indications will represent the sum of the effects of change of temperature and of pressure. If from any independent source we know either of these, we can determine the other. In a sea of uniform temperature throughout its depth, the apparent volume of the liquid would

* Journal of the Chem. Soc. October 1878.

diminish as the pressure increased, and if the temperature increased with the depth, the apparent volume of the liquid would diminish at a slower rate; but it would be always possible to determine the true temperature as long as it did not increase at so great a rate as to dilate the liquid more than it was compressed by the increasing pressure. For the investigation of seas such as the Mediterranean this form of instrument is most valuable. The method of determining accurately both depth and temperature from the combined readings of a mercury and a water piezometer is explained in the paper communicated to the Chemical Society and above referred to.

In the great majority of cases the most convenient instrument to use is the form of Six's thermometer with protected bulb known as the Millar-Casella thermometer, with the following additions and improvements, which Mr Casella has applied to them at my suggestion:—The size of the instrument is increased so that the degrees are wider apart, a degree Fahrenheit on the minimum leg occupying about three millimetres of its length. Besides the scale of degrees which is attached on enamelled slips to the vulcanite at the sides of the stem, there is an arbitrary (millimetre) scale etched on the stem itself. The values of the divisions of this scale are ascertained by a careful comparison with a standard thermometer. It is thus possible to read with certainty to a quarter of a millimetre or a twelfth of a degree Fahrenheit. The errors due to the scale not being rigidly attached to the thermometer, and to the difficulty of determining the height of the index by reference to a scale at the side of instead of over it, are eliminated. Finally, by having the ordinary scale at the sides, the instrument can be used independently of the arbitrary scale, and, even where the arbitrary scale is principally relied on, the scale of degrees enables the observer to know very approximately the true temperature at the moment of observation without reference to tables, and, by noting on every occasion the reading on *both* scales, the chance of errors from misreading is greatly reduced.

The maximum leg, which is only rarely used, is of larger bore than the minimum one; the degrees, therefore, are closer, and the temperature of the instrument may rise as high as 100° F. without the index entering the terminal bulb. This is a detail of consider-

able practical importance, for it is impossible always to protect the thermometers when on deck from the direct rays of the sun, which would speedily disable the maximum side of the thermometer if its range were as limited as that of the minimum one.

It will be seen from what has been said that there is no one instrument which fulfils all conditions required of a perfect deep-sea thermometer. It is necessary, therefore, for the investigator to use his judgment in the selection of the instrument best suited for the particular case before him. In order to be prepared for possibly occurring cases, he should be provided with thermometers of (*a*) the Millar-Casella type, with the improvements just described; (*b*) the mercury piezometer; (*c*) the Negretti & Zambra inverting thermometer. It is well to have several of the first class (*a*), as any number of them can be attached to the line at different depths, and thus much time be saved. In my own practice I generally use four or five at a time. It is not advisable to exceed this number, as the loss in case of accident would be too heavy. Considering the distribution of temperature actually found in lakes and seas of warm and temperate regions, this is the most generally useful instrument when thorough investigation by means of *series* of observations is intended. In the particular and frequently occurring case of an enclosed sea containing a large mass of water showing no variation of temperature when tested by this instrument, it must be replaced by the mercury piezometer (*b*), which possesses the advantage that the position of the thermometric scale shifts along the stem according as the depth varies. Also any number of them can be used at the same time at different depths on the same line. In deep ocean soundings the combination of this instrument with the water piezometer for the determination of both depth and temperature independently of the length of the sounding line is invaluable for accurate work. The inverting thermometer of Messrs Negretti and Zambra (*c*) is the instrument most suitable for isolated observations. It is also of very great use for supplementing and controlling the observations with the other instruments, especially in the case of sea-lochs or fiords, where the temperature distribution is often much disturbed by the imperfect mixture of fresh with salt water.

For the successful and expeditious carrying out of deep-sea temperature observations, the investigator should be furnished with im-

proved Millar-Casella thermometers for the bulk of the work, and the mercury piezometer and the inverting thermometer for particular cases.

All the thermometers, of whatever type, should be carefully compared with a good standard and the results stated in terms of its scale.

4. Preliminary Note on a Crystalline Compound formed in Water containing Sulphuretted Hydrogen and Mercaptan in Solution. By J. Adrian Blaikie, B.Sc.

In the process of making mercaptan by collecting the distillate from a mixture of ethyl-sulphate of calcium and sulphydrate of potassium, along with water and mercaptan, a considerable quantity of crystalline substance was observed to collect in the receiver, and also towards the end of the condenser. The receiver having been placed in a freezing mixture, to condense as much mercaptan as possible, it was thought that the crystalline substance was ice, and the freezing mixture was removed. The crystals, however, continued to be formed, and even stopped up the end of the condenser, so that it was necessary to pour in hot water to melt them. In a few minutes they were again formed, not only in the receiver, but half way up the condensing tubes, through which water at about 2–3° C. was running. As it was evident that these crystals could not be ice, the conditions under which they were formed, and their composition, were subjected to investigation.

The solution of sulphydrate of potassium having been completely saturated with sulphuretted hydrogen at a low temperature, a considerable quantity of that gas was evolved before the formation of mercaptan took place. The crystals were therefore formed in an atmosphere of sulphuretted hydrogen, and as only water and mercaptan were present, could consist of water combined either with one or with both of the other substances.

By pouring a few drops of mercaptan into sulphuretted hydrogen water at 0° C., immediately a few crystals were formed. By passing sulphuretted hydrogen gas into water saturated with mercaptan, and with mercaptan floating on the surface, in a few minutes crystallisation took place, a large amount of sulphuretted hydrogen was absorbed,

and the water thickened into a soft crystalline mass. The quantity of the crystals depended on the amount of mercaptan if sulphuretted hydrogen were in excess, or on the amount of sulphuretted hydrogen if mercaptan were in excess.

On cooling water saturated with mercaptan no such crystalline appearance was observed. As is known from Wöhler's experiments (Annalen der Chemie und Pharmacie, xxxiii. 125), sulphuretted hydrogen only forms a hydrate at $-16°$ C., or under considerable pressure. From these results it is evident that both sulphuretted hydrogen and mercaptan are necessary for the formation of this crystalline compound.

The crystalline mass has much the same appearance as hydrate of chlorine, but is colourless. In the mother liquid the crystals exist for an indefinite time at any temperature below 3° C.; when dried they rapidly melt even at 0° C. Above 3° C. they melt in the mother liquid, with evolution of sulphuretted hydrogen, and formation of a thin layer of mercaptan above the water. Their specific gravity is greater than that of the mother liquid, which in turn is greater than that of ice. By allowing the crystals to stand in this mother liquid, in a corked flask cooled by means of ice-cold water, a crust forms on the surface, which appears to consist of hexagonal plates.

Newly formed crystals when observed under the microscope appear to be prisms, some long and fine, others short and thick, but as they rapidly melt, their form could not be more accurately observed. They dissolve rapidly in absolute alcohol at $-10°$ C., with evolution of a little sulphuretted hydrogen.

A mass of the crystals, when allowed to evaporate slowly, smell strongly of mercaptan, and deposit sulphur. Rapidly heated on platinum foil they suddenly melt, and a gas is given off which burns with a blue flame. The water left becomes milky with separation of sulphur. On further heat being applied the water is evaporated, and the sulphur burnt, without any residue. When dried between well-cooled filter paper, and dissolved in alcohol, with acetate of lead, a dark brown precipitate of sulphide of lead is thrown down, any precipitate of mercaptide of lead being hidden by the darker sulphide.

The presence of mercaptan was distinctly proved by com-

bustion. For combustion the crystals were collected on a funnel fitted with a platinum gauze cone, rapidly washed with ice-cold water, and dried on well-cooled filter paper. When dried as thoroughly as possible, they were rapidly placed in a weighed test tube, cooled in a freezing mixture, weighed, and inserted in an open combustion tube, the lower end of which was at a dull red heat. The combustion tube was filled with a mixture of three-fourths oxide of copper, and one-fourth chromate of lead, with a stream of oxygen passing through it.

The following are the results of analysis :—

I.	·8515 gr.	gave	·8355 H_2O	and	·0525 CO_2
II.	·7655	„	·7530	„	·0380 „
III.	·6820	„	·6620	„	·0365 „

The percentage of carbon is	(1) 1·68 (2) 1·35 (3) 1·46	equivalent to per cent. of mercaptan	(1) 4·33 (2) 3·49 (3) 3·77

The excess of carbon in No. I. is caused probably by the difficulty in obtaining a perfectly dry substance, free from adhering mercaptan.

Direct estimations of sulphur have not as yet proved satisfactory; in some experiments mercaptan was oxidised along with sulphuretted hydrogen, in other cases it was not. It is hoped that shortly a method may be obtained for the more accurate estimation of sulphur in this substance.

From the above results it would appear that the chief constituent of this crystalline compound is water (not less than 90 per cent.), combined with a small quantity of mercaptan and sulphuretted hydrogen.

With sulphide of ethyl, sulphuretted hydrogen, and water, no crystalline compound is formed at 0° C. With sulphide of amyl or with sulphydrate of amyl the results were also negative.

With sulphide of methyl the formation of a crystalline compound is obtained with ease. To a small quantity of water at 2° C. a little pure sulphide of methyl was added, and sulphuretted hydrogen passed in. In a very few minutes crystallisation commenced, and sulphuretted hydrogen was absorbed. These crystals are more stable than those of mercaptan, and it is the writer's intention to study this compound in order to discover whether it has a definite composition.

5. Laboratory Notes. By Professor Tait.

(1.) Measurements of the Electromotive Force of the Gramme Machine at Different Speeds.

The following measurements were made by means of a Gramme machine, recently procured for the University. I desired to make use of it, not only for electric light, but for electrolysis, the exciting of electromagnets, and various other purposes for which we have hitherto used from 4 to 40 or so Bunsen cells. I therefore arranged the driving-gear so that with the same motor (a $3\frac{1}{2}$ h.p. gas-engine) it was easy to use either of three speeds. These are, approximately, 800, 533, and 320 turns per minute. The electromotive force varies, of course, not only with the speed but with the resistance of the whole circuit—falling off at first rapidly and then more slowly for any one speed, as the resistance is increased. As I had no means of measuring the speed *directly*, I was somewhat puzzled at first to find the electromotive force at any one speed rise to a maximum, and then rapidly fall off as the whole resistance was gradually diminished. But I soon found that this was due in great part to the slipping of the driving-belts (though they were very tight), whenever the intensity of the current exceeded a certain amount. A liberal use of rosin almost removed this anomaly, though there is reason to believe there is still considerable slipping.

The following table gives the average of a number of experiments which accord fairly with one another. The resistance of the conductor of the Gramme machine is about 1·16 B. A. units. For the added resistance I used coils of stout covered copper wire which were in the laboratory, having resistances 0·054, 1·844, and 3·63, respectively. The first was always in circuit, and a portion of it, of resistance 0·0015, was introduced into the circuit of a galvanometer having a resistance 23. The deflections of the galvanometer were observed with the first coil alone in the circuit of the Gramme, then with the addition of the second or third, and finally with all the coils.

The explosions in the gas-engine occur only at every *second* stroke of the piston. This and other causes render the driving power not absolutely steady, but the *average* deflection of the galvanometer was very easily observed.

From the graphic representation of the results the following numbers were taken :—

Nominal Speed.	Whole Resistance.	Electromotive Force in terms of a Bunsen cell.
800	1·5	38*
...	3·	38
...	4·5	36
...	6	31
533	1·5	24
...	3·	23
...	4·5	17·5
...	6·	9·
320	1·5	13
...	3·	5
...	4·5	2·5
...	6	2·

Next, instead of the second or third coils above mentioned, a Duboscq's lamp was included in the circuit, the other arrangements being as before, and the speed being 800 nominal. The deflection corresponded to an electromotive force of about 39 Bunsen cells, and a resistance of 2·66. As the lamp itself was sometimes found to have a resistance of as much as 0·6, and as the carbons have a resistance of from 0·115 to 0·045, per 4 inches, it appears that, approximately, the resistance of the electric arc, under these conditions, is at least 0·8.

Subsequent experiments, in which the lamp was not used, gave resistances varying from 0·75 to 1·2, according to the length of the arc —and when a little sodium was introduced, it fell to 0·25. These estimates, of course, include the effect due to heating and pointing the carbons.

The want of accurate speed determinations, of course, deprives these results of scientific value, but they are very useful as an expression of the electromotive force practically to be obtained from the Gramme machine under different circumstances of its ordinary working,—showing, as they do, what adjustments to make for the purposes of a particular experiment.

* This particular number must be over-estimated, for about 5 h.p. is required to maintain an electromotive force of 38 Bunsens in a resistance 1·5.

The very rapid increase of electromotive force with diminished resistance at the lowest speed, seems to show that the speed is very considerably overrated when stated as 800 or 533, with the resistance between 1 and 2 B. A. units. I hope soon to have the means of accurately measuring the speed realised, and shall then repeat these experiments for a scientific, and not a mere practical purpose.

(2.) On the Law of Extension of India-rubber at Different Temperatures.

To fill the vacancies in Foreign Honorary Fellowships caused by the deaths of Claude Bernard, Elias Magnus Fries, Henri Victor Regnault, Angelo Secchi, the following Gentlemen were elected:—

Frank Cornelius Donders, Utrecht.
Asa Gray, Cambridge, U.S.
Jules Janssen, Paris.
Johann Benedict Listing, Gottingen.

The following Gentlemen were duly elected Fellows of the Society:—

Thomas H. Cockburn Hood, F.G.S., Junior Carlton Club, Pall Mall.
Thomas Gilray, M.A., 6 Carlung Place, Edinburgh.
Alex. Bennett M'Grigor, LL.D., 19 Woodside Terrace, Glasgow.
James Blaikie, M.A., 14 Viewforth Place, Edinburgh.

Monday, 17th March 1879.

Professor KELLAND, President, in the Chair.

The following Communications were read:—

1. On Gravitational Oscillations of Rotating Water. By Sir William Thomson.

(*Abstract.*)

This is really Laplace's subject in his Dynamical Theory of the Tides; where it is dealt with in its utmost generality except one important restriction,—the motion of each particle to be infinitely nearly horizontal, and the velocity to be always equal for all par-

ticles in the same vertical. This implies that the greatest depth must be small in comparison with the distance that has to be travelled to find the deviation from levelness of the water-surface altered by a sensible fraction of its maximum amount. In the present short communication I adopt this restriction; and farther, instead of supposing the water to cover the whole or a large part of the surface of a solid spheroid as does Laplace, I take the simpler problem of an area of water so small that the equilibrium-figure of its surface is not sensibly curved. Imagine a basin of water of any shape, and of depth, not necessarily uniform, but, at greatest, small in comparison with the least diameter. Let this basin and the water in it rotate round a vertical axis with angular velocity ω so small that the greatest equilibrium slope due to it may be a small fraction of the radian: in other words, the angular velocity must be small in comparison with $\sqrt{\frac{g}{\frac{1}{2}\mathrm{A}}}$, where g denotes gravity, and A the greatest diameter of the basin. The equations of motion are

$$\left.\begin{aligned} \frac{du}{dt} - 2\omega v &= -\frac{1}{\rho}\frac{dp}{dx} \\ \frac{dv}{dt} + 2\omega u &= -\frac{1}{\rho}\frac{dp}{dy} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where u and v are the component velocities of any point of the fluid in the vertical column through the point (xy), relatively to horizontal axes Ox Oy revolving with the basin; p the pressure at any point x, y, z, of this column; and ρ the uniform density of the liquid. The terms $\omega^2 x$, $\omega^2 y$, which appear in ordinary dynamical equations referred to rotating axes represent components of centrifugal force, and therefore do not appear in these equations. Let now D be the mean depth and D + h the actual depth at any time t in the position (xy). The "equation of continuity" is

$$\frac{d(\mathrm{D}u)}{dx} + \frac{d(\mathrm{D}v)}{dy} = -\frac{dh}{dt} \cdot \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Lastly, by the condition that the pressure at the free surface is

constant, and that the difference of pressures at any two points in the fluid is equal to $g\ \times$ difference of levels, we have

$$\left.\begin{aligned}\frac{dp}{dx}&=g\varrho\frac{dh}{dx}\\[2ex] \frac{dp}{dy}&=g\varrho\frac{dh}{dy}\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Hence for the case of gravitational oscillations (1) become

$$\left.\begin{aligned}\frac{du}{dt}-2\omega v&=-g\frac{dh}{dx}\\[2ex] \frac{dv}{dt}+2\omega u&=-g\frac{dh}{dy}\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (4)$$

From (1) or (4) we find by differentiation, &c.

$$\frac{d}{dt}\left(\frac{dv}{dx}-\frac{du}{dy}\right)+2\omega\left(\frac{du}{dx}+\frac{dv}{dy}\right)=0 \quad . \quad . \quad . \quad (5)$$

which is the equation of vortex motion in the circumstances.

These equations reduced to polar coordinates, with the following notation,—

$$x=r\cos\theta\,,\quad y=r\sin\theta$$
$$u=\zeta\cos\theta\tau\quad\sin\theta\,,\quad v=\zeta\sin\theta+\tau\cos\theta\,,$$

become

$$\frac{\mathrm{D}\zeta}{r}+\frac{d(\mathrm{D}\zeta)}{dr}+\frac{d(\mathrm{D}\tau)}{rd\theta}=-\frac{dh}{dt} \quad . \quad . \quad . \quad . \quad (2')$$

$$\left.\begin{aligned}\frac{d\zeta}{dt}-2\omega\tau&=-g\frac{dh}{d\zeta}\\[2ex] \frac{d\tau}{dt}+2\omega\zeta&=-g\frac{dh}{rd\theta}\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (4')$$

$$\frac{d}{dt}\left(\frac{\tau}{r}+\frac{d\tau}{dr}-\frac{d\zeta}{rd\theta}\right)+2\omega\left(\frac{\zeta}{r}+\frac{d\zeta}{dr}+\frac{d\tau}{rd\theta}\right)=0 \quad . \quad (5')$$

In these equations D may be any function of the coordinates. Cases of special interest in connection with Laplace's tidal equations are had by supposing D to be a function of r alone. For the present,

however, we shall suppose D to be constant. Then (2) used in (5) or (2′) in (5′) gives after integration with respect to t

$$\frac{dv}{dx} - \frac{du}{dy} = 2\omega\frac{h}{\mathrm{D}} \qquad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

or in polar coordinates

$$\frac{\tau}{r} + \frac{d\tau}{dr} - \frac{d\zeta}{rd\theta} = 2\omega\frac{h}{\mathrm{D}} \qquad . \quad . \quad . \quad . \quad . \quad (6')$$

These equations (6) (6′) are instructive and convenient though they contain nothing more than is contained in (2) or (2′), and (4) or (4′).

Separating u and v in (4), or ζ and τ in (4′), we find

$$\left.\begin{aligned}\frac{d^2u}{dt^2} + 4\omega^2u &= -g\left(\frac{d}{dt}\frac{dh}{dx} + 2\omega\frac{dh}{dy}\right)\\ \text{and}\qquad \frac{d^2v}{dt^2} + 4\omega^2v &= g\left(2\omega\frac{dh}{dx} - \frac{d}{dt}\frac{dh}{dy}\right)\end{aligned}\right\} \qquad . \quad . \quad (7)$$

or in polar coordinates

$$\left.\begin{aligned}\frac{d^2\zeta}{dt^2} + 4\omega^2\zeta &= -g\left(\frac{d}{dt}\frac{dh}{dr} + 2\omega\frac{dh}{rd\theta}\right)\\ \frac{d^2\tau}{dt^2} + 4\omega^2\tau &= g\left(2\omega\frac{dh}{dr} - \frac{d}{dt}\frac{dh}{rd\theta}\right)\end{aligned}\right\} \qquad . \quad . \quad (7')$$

Using (7) (7′), in (2) (2′), with D constant, or in (6) (6′) we find—

$$g\mathrm{D}\left(\frac{d^2h}{dx^2} + \frac{d^2h}{dy^2}\right) = \frac{d^2h}{dt^2} + 4\omega^2h \qquad . \quad . \quad . \quad (8)$$

and

$$g\mathrm{D}\left(\frac{d^2h}{dr^2} + \frac{1}{r}\frac{dh}{dr} + \frac{d^2h}{rd\theta^2}\right) = \frac{d^2h}{dt^2} + 4\omega^2h \qquad . \quad . \quad (8')$$

It is to be remarked that (8) and (8′) are satisfied with u or v substituted for h.

I. Solutions for Rectangular Coordinates.

The general tpye-solution of (8) is $h = \epsilon^{ax}\epsilon^{\beta y}\epsilon^{\gamma t}$ where a, β, γ, are connected by the equation

$$a^2 + \beta^2 = \frac{\gamma^2 + 4\omega^2}{g\mathrm{D}} \qquad . \quad . \quad . \quad . \quad . \quad (9)$$

For waves or oscillations we must have $\gamma = \sigma\sqrt{-1}$ where σ is real.

I*a*. *Nodal Tesseral Oscillations.*

For nodal oscillations of the tesseral type we must have $\alpha = m\sqrt{-1}$, $\beta = n\sqrt{-1}$ where m and n are real, and by putting together properly the imaginary constituents we find

$$h = \mathrm{C} \begin{matrix}\sin\\ \cos\end{matrix} \sigma t \begin{matrix}\sin\\ \cos\end{matrix} mx \begin{matrix}\sin\\ \cos\end{matrix} ny \quad . \quad . \quad . \quad . \quad (10),$$

where m, n, σ are connected by the equation

$$m^2 + n^2 = \frac{\sigma^2 - 4\omega^2}{g\mathrm{D}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (11).$$

Finding the corresponding values of u and v, we see what the boundary conditions must be to allow these tesseral oscillations to exist in a sea of any shape. No bounding line can be drawn at every part of which the horizontal component velocity perpendicular to it is zero. Therefore to produce or permit oscillations of the simple harmonic type in respect to form, water must be forced in and drawn out alternately all round the boundary, or those parts of it (if not all) for which the horizontal component perpendicular to it is not zero. Hence the oscillations of water in a rotating rectangular trough are not of the simple harmonic type in respect to form, and the problem of finding them remains unsolved.

If $\omega = 0$, we fall on the well-known solution for waves in a non-rotating trough, which are of the simple harmonic type.

I*b*. *Waves or Oscillations in an endless Canal with straight parallel sides.*

For waves in a canal parallel to x, the solution is

$$h = \mathrm{H}\epsilon^{-ly} \cos(mx - \sigma t) \quad . \quad . \quad . \quad . \quad . \quad (12),$$

where l, m, σ satisfy the equation

$$m^2 - l^2 = \frac{\sigma^2 - 4\omega^2}{g\mathrm{D}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (13),$$

in virtue of (9) or (11).

Using these in (7) we find that v vanishes throughout if we make

$$l = \frac{2\omega m}{\sigma} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14) ;$$

and with this value for l in (12), we find, by (7),

$$u = \mathrm{H}\frac{gm}{\sigma}\epsilon^{-ly}\cos(mx - \sigma t) \quad . \quad . \quad . \quad (15) :$$

and using (14) and (13) we find

$$m^2 = \frac{\sigma^2}{g\mathrm{D}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16),$$

from which we infer that the velocity of propagation of waves is the same for the same period as in a fixed canal. Thus the influence of rotation is confined to the effect of the factor $\epsilon^{-2\omega m/\sigma . y}$. Many interesting results follow from the interpretation of this factor with different particular suppositions as to the relation between the period of the oscillation $\left(\frac{2\pi}{\sigma}\right)$, the period of the rotation $\left(\frac{2\pi}{\omega}\right)$, and the time required to travel at the velocity $\frac{\sigma}{m}$ across the canal. The more approximately nodal character of the tides on the north coast of the English Channel than on the south or French coast, and of the tides on the west or Irish side of the Irish Channel than on the east or English side, is probably to be accounted for on the principle represented by this factor, taken into account along with frictional resistance, in virtue of which the tides of the English Channel may be roughly represented by more powerful waves travelling from west to east, combined with less powerful waves travelling from east to west, and those of the southern part of the Irish Channel by more powerful waves travelling from south to north combined with less powerful waves travelling from north to south. The problem of standing oscillations in an endless rotating canal is solved by the following equations—

$$\left.\begin{aligned} h &= \mathrm{H}\,\{\epsilon^{-ly}\cos(mx - \sigma t) - \epsilon^{ly}(\cos mx + \sigma t)\} \\ u &= \mathrm{H}\frac{gm}{\sigma}\{\epsilon^{-ly}\cos(mx - \sigma t) + \epsilon^{ly}\cos(mx - \sigma t)\} \\ v &= 0 \end{aligned}\right\} \quad . \quad (17)$$

If we give ends to the canal we fall upon the unsolved problem referred to above of tesseral oscillations. If instead of being rigorously straight we suppose the canal to be circular and endless, provided the breadth of the canal to be small in comparison with the radius of the circle, the solution (17) still holds. In this case, if c denote the circumference of the canal, we must have $m=\frac{2i\pi}{c}$, where i is an integer.

II. Oscillations and Waves in Circular Basin (Polar Coordinates).

Let

$$h = \mathrm{P}\cos(i\theta - \sigma t) \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

be the solution for height, where P is a function of r. By (8′) P must satisfy the equation

$$\frac{d^2\mathrm{P}}{dr^2}+\frac{1}{r}\frac{d\mathrm{P}}{dr}-\frac{i^2\mathrm{P}}{r^2}+\frac{\sigma^2-4\omega^2}{g\mathrm{D}}\mathrm{P}=0 \quad . \quad . \quad . \quad . \quad (19)$$

and by (7′) we find

$$\left.\begin{aligned}\zeta &= \frac{g}{\sigma^2-4\omega^2}\sin(i\theta-\sigma t)\left(\sigma\frac{d\mathrm{P}}{dr}-2\omega i\,\frac{\mathrm{P}}{r}\right)\\ \tau &= \frac{-g}{\sigma^2-4\omega^2}\cos(i\theta-\sigma t)\left(2\omega\frac{d\mathrm{P}}{dr}-\sigma i\frac{\mathrm{P}}{r}\right)\end{aligned}\right\} \quad . \quad . \quad (20)$$

This is the solution for water in a circular basin, with or without a central circular island. Let a be the radius of the basin, and if there be a central island let a' be its radius. The boundary conditions to be fulfilled are $\zeta=0$, when $r=a$, and when $r=a'$. The ratio of one to the other of the two constants of integration of (19), and the speed σ of the oscillation, are the two unknown quantities to be found by these two equations. The ratio of the constants is immediately eliminated, and the result is a transcendental equation for σ. There is no difficulty, only a little labour, in thus finding as many as we please of the fundamental modes, and working out the whole motion of the system for each. The roots of this equation, which are found to be all real by the Fourier-Sturm-Liouville-theory,

are the speeds * of the successive fundamental modes, corresponding to the different circular nodal subdivisions of the i diametral divisions implied by the assumed value of i. Thus, by giving to i the successive values 0, 1, 2, 3, &c., and solving the transcendental equation so found for each, we find all the fundamental modes of vibration of the mass of matter in the supposed circumstances.

If there is no central island, the solution of (19) which must be taken, is that for which P and its differential coefficients are all finite when $r=0$. Hence P is what is called a Bessel's function of the first kind and of order i; and according to the established notation † we have

$$\mathrm{P}=\mathrm{J}_i\left(r\sqrt{\frac{\sigma^2-\omega^2}{g\mathrm{D}}}\right) \quad . \quad . \quad . \quad (21)$$

The solution found above for an endless circular canal is fallen upon by giving a very great value to i. Thus, if we put $\frac{2\pi r}{i}=\lambda$ so that λ may denote wave-length, we have $\frac{i}{r}=\frac{2\pi}{\lambda}$, which will now be the m of former notation. We must now neglect the term $\frac{1}{r}\frac{dh}{dr}$ in (19), and thus the differential equation becomes

$$\frac{d^2h}{dr^2}+\left(\frac{\sigma^2-4\omega^2}{g\mathrm{D}}-m^2\right)h=0\,,$$

or

$$\frac{d^2h}{dr^2}-l^2h=0 \quad . \quad . \quad . \quad . \quad (22),$$

where l^2 denotes $m^2-\frac{\sigma^2-4\omega^2}{g\mathrm{D}}$. A solution of this equation is $h=c\epsilon^{-ly}$ where $y=a-r$, and using this in (20) above, we find $\zeta=\frac{-g}{\sigma^2-4\omega^2}\mathrm{C}\sin(mx-\sigma t)(\sigma l-2\omega m)\epsilon^{-ly}$, where $mx=i\theta$. Hence, to make $\zeta=0$ at each boundary, we have $\sigma l=2\omega m$, which makes

* In the last two or three tidal reports of the British Association the word "speed," in reference to a simple harmonic function, has been used to designate the angular velocity of a body moving in a circle in the same period. Thus, if T be the period $\frac{2\pi}{\mathrm{T}}$ is the speed; *vice versa*, if σ be the speed $\frac{2\pi}{\sigma}$ is the period.

† Neumann, "Theorie der Bessel'schen Functionen" (Leipzig, 1867), § 5; and Lommel, "Studien über die Bessel'schen Functionem" (Leipzig 1868), § 29.

$\zeta=0$, not only at the boundaries, but throughout the space for which the approximate equation (22) is sufficiently nearly true. And, putting for l^2 its value above, we have

$$4\omega^2 m^2 = \sigma^2\left(m^2 - \frac{\sigma^2 - 4\omega^2}{g\mathrm{D}}\right);$$

whence

$$m^2 = \frac{\sigma^2}{g\mathrm{D}},$$

which agrees with (16) above.

I hope in a future communication to the Royal Society to go in detail into particular cases, and to give details of the solutions at present indicated, some of which present great interest in relation to tidal theory, and also in relation to the abstract theory of vortex motion. The characteristic differences between cases in which σ is greater than 2ω, or less than 2ω, are remarkably interesting, and of great importance in respect to the theory of diurnal tides in the Mediterranean, or other more or less nearly closed seas in middle latitudes, and of the lunar fortnightly tide of the whole ocean. It is to be remarked that the preceding theory is applicable to waves or vibrations in any narrow lake or portion of the sea covering not more than a few degrees of the earth's surface, if for ω we take the component of the earth's angular velocity round a vertical through the locality, that is to say, $\omega = \gamma \sin l$, where γ denotes the earth's angular velocity, and l the latitude.

2. On the Effects of Chloroform, Ethidene Dichloride, and Ether on Blood-Pressure. By Joseph Coats, M.D., William Ramsay, Ph.D., and John G. M'Kendrick, M.D., the University of Glasgow. Communicated by Professor M'Kendrick.

Abstract.

Dr Coats stated that this communication referred to part of an investigation on the physiological action of anæsthetics, undertaken, at the request of the British Medical Association, by Dr Ramsay, Dr M'Kendrick, and himself. After describing the method of obtaining accurate tracings of variations in blood-pressure by means of a kymograph, he stated that the facts obtained from these re-

searches seemed to the Committee to warrant the following conclusions:—

1. Both chloroform and ethidene administered to animals have a decided effect in reducing the blood-pressure, while ether has no appreciable effect of this kind.

2. Chloroform reduces the pressure much more rapidly and to a greater extent than ethidene.

3. Chloroform has sometimes an unexpected and apparently capricious effect on the heart's action, the pressure being reduced with great rapidity almost to *nil*, while the pulsations are greatly retarded or even stopped. The occurrence of these sudden and unlooked-for effects on the heart's action seems to be a source of serious danger to life, all the more that in two instances they occurred more than a minute after chloroform had ceased to be administered, and after the recovery of the blood-pressure.

4. Ethidene reduces the blood-pressure by regular gradations and not, so far as observed, by these sudden and unexpected depressions.

5. Chloroform may cause death in dogs either by primarily paralysing the heart or the respiratory mechanism. The variations in this respect seem to depend to some extent on individual peculiarities of the animals; in some the cardiac centres are more readily affected, in others the respiratory. But peculiarities in the condition of the same animal very probably have some effect in determining the vulnerability of these two centres respectively, and they may both fail simultaneously.

6. In most cases respiration stops before the heart's action, but there was one instance in which respiration continued while the heart had stopped, and only failed a considerable number of seconds after the heart had resumed.

7. The use of artificial respiration was very effective in restoring animals in danger of dying from the influence of chloroform. In one instance its prolonged use produced recovery even when the heart had ceased beating for a considerable time.

8. Under the use of ethidene there was on no single occasion an absolute cessation either of the heart's action or of respiration, although they were sometimes very much reduced. It can therefore be said that, though not free from danger on the side of the

heart and respiration, this agent is, in a very high degree, safer than chloroform.

9. These results confirm and amplify those stated in a previous report, to the effect that ethidene does not compromise the heart as does chloroform. By the method of experimentation then employed, the effect on the blood-pressure could not be determined, and altogether the results here obtained are more exact and unequivocal.

3. Experiments with Rotating Discs. By Mr John Aitken. Communicated by Professor M'Kendrick.

4. General Theorems on Determinants. By Thomas Muir, M.A.

5. Preliminary Note on Alternants. By Thomas Muir, M.A.

(*Abstract.*)

When the elements of the first row of a determinant are all positive integral powers of one quantity, the elements of the second the like positive integral powers of another quantity, and so on, the determinant is called an ALTERNANT; for example,

$$\begin{vmatrix} a^m & a^n & a^p \\ b^m & b^n & b^p \\ c^m & c^n & c^p \end{vmatrix}$$

Every alternant of the n^{th} degree is evidently a function of n variables, viz., the n quantities whose powers are the elements. To interchange two of these variables would be the same as to interchange two of the rows of the determinant, and therefore would have the effect of merely changing the sign of the function. A function having this property, and therefore closely resembling a symmetric function, Cauchy called a symmetric function also, distinguishing the two kinds as *alternating* and *permanent*. The narrower meaning of symmetric having, however, been adhered to, the other kind of function, viz., that above exemplified, has been known as simply an *alternating function*, and hence Sylvester's word *alternant*.

Now it is well known that the alternant whose indices are in order 0, 1, 2, 3, ... is equal to the difference-product of its variables. In regard to every other alternant it is evident that it must contain the said difference-product as a factor, but what the co-factor should be is not so readily seen. In particular cases, doubtless, it can be found without much difficulty, but a general method of obtaining it has hitherto been a desideratum. Such a general method the author has discovered along with a number of less important results, bearing on the same special form of determinant.

Monday, 7th April 1879.

SIR ALEXANDER GRANT, BART., Vice-President, in the Chair.

1. Professor Geddes's Theory of the "Iliad." By Professor Blackie.

(*Abstract.*)

Professor Blackie, after paying a high compliment to the erudition, ingenuity, and fine taste of the Aberdeen Hellenist, proceeded to give reasons why, in his opinion, the theory now broached, to the effect that certain books of the "Iliad" were composed by the author of the "Odyssey," which author is to be considered as the real Homer, though not destitute of a certain plausibility, is untenable. The reasons were—(1.) The character of the minstrel as distinguished from the literary epos warrants the presumption that any small diversity in certain secondary characteristics of different sections of the poem, as we now have it, is a legitimate proof, not of diversity of authorship, but only of diversity of materials collected from different sources. (2.) The manner in which the minstrel epos was originally circulated, not as a separate literary composition to be read and studied, but as a sequence of easily separable cantos to be handed about and sung separately, rendered it, even when wrought into a finished artistic whole by the genius of a great singer, peculiarly liable to interpolations and variations of various kinds, which form no legitimate ground of induction with regard to the character

or attitude of the original composer. (3.) Not a few of the most prominent differential features emphasized by Professor Geddes are to be looked rather as the two sides of one rich mind than as the diverse workmanship of two different minds. A great poet will be as tender on one occasion as he is fierce on another, and Goethe is not the less author of "Torquato Tasso" because he is the author of "Faust." (4.) Not the least objection to the books exsected by Mr Grote, and appropriated by Professor Geddes, being attributed to the author of the "Odyssey," is the fact that some of these books are at once the most poetically impressive and the most fully charged with the fervour peculiar to the "Iliad," and, if they are not absolutely necessary to what Mr Grote calls the logical sequence of the poem, do certainly contribute most largely to its effect as a work of art. (5.) Some of the differential features dwelt on by the Aberdeen Professor are either too slight, too sparse, and too equivocal to warrant any sure induction, or are explained most naturally by the character and tone of the poem, and the nature of the subject. In the quiet books of the "Iliad" some things would naturally occur that are more kindred to the gentle tone of the "Odyssey," than the fervid and somewhat fierce tone and contents of those books of the "Iliad" where Achilles is the dominant figure. (6.) Lastly, the theory of development in moral and religious matters applied by Professor Geddes to the "Odyssey," and what he calls the Odyssean books of the "Iliad," Professor Blackie felt himself compelled flatly to deny. Jove is in every respect as stern and just a moral governor of the world in the "Iliad" as in the "Odyssey;" and the haughtiness of Agamemnon meets with its retribution, as publicly and as prominently in the one poem, as does the insolence of the Suitors in the other. In the "Iliad," Zeus is the steward of national war—*ταμιας πολέμοιο*; in the "Odyssey" the avenger of domestic wrong (*ξένιος*); but in both poems he is equally moral in great cosmical matters, and equally immoral, as it strikes us, in certain small personal matters. There are no palæozoic and neozoic periods of theological belief to warrant the assumption of successive stages of moral development in different portions of the Homeric poems.

At the close of Professor Blackie's paper, Professor Campbell, St Andrews, remarked that before reading the book of Professor Geddes he looked upon Grote's theory with some doubt and suspicion, but

after reading the book his opinion was modified. He thought Professor Geddes had strengthened the case for Grote's theory, though not to the extent of proving an affirmative.

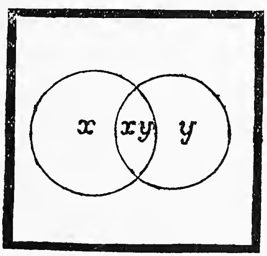
Dr Donaldson concurred with the views of Professor Blackie.

Professor Sellar, while having the greatest admiration for Professor Geddes's knowledge and ingenuity, felt that he had done nothing in the way of enlightening those interested in the subject.

Professor Blackie, in replying, observed that the Society were under obligations to Professor Geddes for having raised the question.

2. The Principles of the Algebra of Logic. Part III.—Application to certain Problems in the Theory of Probability. By Dr Alexander Macfarlane.

The Algebra of Logic, being the science of Necessity and Probability, supplies a variety of methods of great power for solving problems in the theory of Probability. I propose to bring before the Society a few examples extracted from my work on the "Principles of the Algebra of Logic," which is about to be published. A large class of problems, some of which have created considerable diversity of opinion among mathematicians of eminence, can be solved by means of a single theorem. It consists in finding the arithmetical value of $\frac{xy}{x}$. The meaning of this expression is shown by the diagram—



The collection of individuals forming the subject of discourse is represented by the part of the page within the square, those which have a character x by the part inside the one circular line, and those which have the character y by the part inside the other

circular line; those having both x and y by the part inside both lines, those having neither by the part outside.

$$\text{Now} \qquad xy = xy \qquad \therefore \; y = \frac{xy}{x}.$$

Expand $\frac{xy}{x}$ in terms of the parts formed by means of xy and x.

$$\frac{xy}{x} = a\,xy\,x + b\,xy\,(1-x) + c(1-xy)\,x + d(1-xy)(1-x).$$

$$\text{Let } xy = 1 \text{ and } x = 1. \quad \text{Then, } a = \frac{1}{1} = 1.$$

$$xy = 1 \text{ and } x = 0, \text{ then } b = \frac{1}{0}.$$

$$xy = 0 \text{ and } x = 1, \text{ then } c = \frac{0}{1} = 0.$$

$$xy = 0 \text{ and } x = 0, \text{ then } d = \frac{0}{0}.$$

$$\therefore \frac{xy}{x} = xy\,x + 0(1-xy)x + \frac{0}{0}(1-xy)(1-x),$$

and also $xy(1-x) = 0$, as evidently ought to be the case.

By putting in

$$x^2 = x, \text{ or } x(1-x) = 0,$$

we get
$$\frac{xy}{x} = xy + 0\,x(1-y) + \frac{0}{0}\overline{(1-x)},$$

that is, what is y is identical with what is x and y, together with no part of what is x and not y, together with an indefinite part of what is not x. The truth of this is evidenced by the diagram. Since every logical equation is true arithmetically,

$$\bar{y} = \overline{xy} + \frac{0}{0}(1-x);$$

where the bar denotes that the arithmetical value of the symbol is taken.

Applications of the above Theorem.

(1.) The probability that it thunders upon a given day is p, the probability that it both thunders and hails is q, but of the connection of the two phenomena of thunder and hail, nothing further is supposed to be known. Required the probability that it hails on the proposed day.

Let U = a succession of states of the atmosphere at a given place, an individual state being of the length of a day.

x = containing a thunderstorm,
y = containing a hailstorm.

Then the data are—

$$x = \bar{p}\,, \qquad xy = \bar{q}\,.$$

Hence, by means of the theorem proved

$$\bar{y} = \bar{q} + \frac{0}{0}(\bar{1} - \bar{p})\,,$$

$$\therefore\quad \bar{y} > \bar{q}\,,$$

$$\text{and } < \bar{q} + \bar{1} - p\,.$$

(2.) A says that B says that a certain event took place; required the probability that the event did take place, p_1 and p_2 being A's and B's respective probabilities of speaking the truth.

The solution of this problem recently gave rise to a great amount of discussion in the "Educational Times." No fewer than four different solutions are given, viz. :—

Todhunter—

$$p_1p_2 + (1 - p_1)(1 - p_2)\,.$$

Artemas Martin—

$$p_1\{p_1p_2 + (1 - p_1)(1 - p_2)\}\,.$$

Woolhouse and American mathematicians—

$$p_1p_2\,.$$

Cayley—

$$p_1p_2 + \beta(1 - p_1)(1 - p_2) + \kappa(1 - \beta)(1 - p_1)\,,$$

where β is the chance, on the supposition of the incorrectness of A's statement, that B told A that the event did *not* happen, and $1 - \beta$ that he did not tell him at all. κ is the antecedent probability.

Let U = statements of A about B's statements about an event taking place.

x = which truly reported a statement by B,
y = which truly reported the event.

Then

$$x = \bar{p}_1, \quad \text{and } xy = \bar{p}_1 \bar{p}_2,$$

therefore, by means of the theorem proved

$$y = \bar{p}_1 \bar{p}_2 + \frac{0}{0} (\bar{1} - \bar{p}_1),$$

$$> \bar{p}_1 \bar{p}_2,$$

$$< \bar{p}_1 \bar{p}_2 + \bar{1} - \bar{p}_1.$$

Todhunter assumes that $\frac{0}{0} = 1 - p_2$; and Woolhouse that $\frac{0}{0} = 0$.

The above solution contradicts the first three, and agrees with the fourth, without introducing more than one unknown quantity. But, by means of the theorem referred to, we can find the solution when there are n persons involved in the tradition.

(3.) A_1 says that A_2 says that A_3 says . . . that A_n says a certain event took place. The probabilities of A_1, A_2, . . . A_n speaking the truth are p_1, p_2, . . . p_n respectively. Required the probability that the event took place.

Let—

U = series of statements of A_1 about A_2 saying &c.,
x_1 = which truly reported a statement of A_2,
x_2 = " " A_3,
. . . . . .
x_n = " " the event.

Now,

$$x_n = \frac{x_1 x_2 \ldots x_n}{x_1 x_2 \ldots x_{n-1}}$$

$$= x_1 x_2 \ldots x_n + \frac{0}{0} (1 - x_1 x_2 \ldots x_{n-1}) \text{ by the theorem,}$$

$$= \overline{p_1 p_2} \ldots \overline{p_n} + \frac{0}{0} (\bar{1} - \overline{p_1 p_2 \ldots p_{n-1}}) \text{ by the data.}$$

Cor. 1. Suppose that each always reports truly. Then

$$x_n = 1 + \frac{0}{0} \times 0 = 1.$$

Cor. 2. Suppose that each always reports truly excepting A_n. Then

$$x_n = \bar{p}_n.$$

Cor. 3. Suppose that A_n always speaks falsely, then

$$x_n = \frac{0}{0}(\bar{1} - \overline{p_1 p_2 \ldots p_{n-1}}) .$$

Cor. 4. Suppose that any other than A_n always speaks falsely, then

$$x_n = \frac{0}{0};$$

that is, the probability is quite indefinite.

Cor. 5. Suppose that each $\bar{p} = \frac{1}{2}$,

then,

$$x_n = \left(\frac{1}{2}\right)^n + \frac{0}{0}\left(1 - \left(\frac{1}{2}\right)^{n-1}\right);$$

which, when n is infinite is equal to $\frac{0}{0}$, that is, is perfectly indefinite.

(4.) A goes to hall p times in q consecutive days and sees B there r times. What is the most probable number of times that B was in the hall in the q days?—*Whitworth's Choice and Chance.*

$$\begin{aligned} U &= \text{the consecutive days;} \\ x &= \text{on which A goes to hall;} \\ y &= \text{on which B goes to hall.} \end{aligned}$$

The data are—

$$U = \bar{q};$$

$$Ux = \bar{p}; \qquad\qquad Uxy = \bar{r};$$

∴ by means of the theorem,

$$Uy = \bar{r} + \frac{0}{0}(\bar{q} - \bar{p}) .$$

To find the most probable value, make the assumption of independence, that is, that B is as likely to go to hall when A does not go, as when A does go.

Then the most probable value of Uy is

$$r + \frac{r}{p}(q - p) = \frac{rq}{p} .$$

Whitworth gives $\frac{(q + 1)r}{p}$, or next lower integer.

Cor. Let $\bar{r} = \bar{p} = \bar{q}$. Then $Uy = \bar{q}$.

Problems discussed by Boole in the "Laws of Thought."

1. The probability that one or both of two events happen is $\bar{p}$, that one or both of them fail is $\bar{q}$. What is the probability that only one of these happens?

$$xy + x(1-y) + (1-x)y = \bar{p},$$
$$x(1-y) + (1-x)y + (1-x)(1-y) = \bar{q},$$

it is required to find

$$x(1-y) + y(1-x).$$

Let

$$x(1-y) + y(1-x) = a + b\{xy + x(1-y) + (1-x)y\} + c\{x(1-y) + (1-x)y + (1-x)(1-y)\}.$$

$$\begin{array}{llll}
\text{Let } x = 1 & y = 1 & \text{then} & 0 = a + b \\
\text{,,} \quad x = 1 & y = 0 & \text{,,} & 1 = a + b + c \\
\text{,,} \quad x = 0 & y = 1 & \text{,,} & 1 = a + b + c \\
\text{,,} \quad x = 0 & y = 0 & \text{,,} & 0 = a \qquad + c.
\end{array}$$

We have four equations, but two of them are identical. When solved—

$$a = -1, \qquad b = 1, \qquad c = 1,$$
$$\therefore \quad x(1-y) + y(1-x) = -1 + \bar{p} + \bar{q}.$$

This method by indeterminate coefficients serves to indicate whether a problem is determinate. For example, investigate the first problem by its means—

$$y = a + bx + cxy.$$

Then

$$\begin{aligned}
1 &= a + b + c, \\
0 &= a + b, \\
\left.\begin{aligned} 1 &= a \\ 0 &= a \end{aligned}\right\} &;
\end{aligned}$$

$$\therefore \quad a = \frac{0}{0}, \qquad b = -\frac{0}{0}, \qquad c = 1.$$

$$\therefore \quad y = \frac{0}{0} - \frac{0}{0}x + xy$$
$$= xy + \frac{0}{0}(1-x).$$

2. The probabilities of two causes A_1 and A_2 are $\bar{a}$ and $\bar{b}$ respec-

tively. The probability that if the cause A_1 present itself, an event E will accompany it (whether as a consequence of the cause A_1 or not) is p_1, and the probability that if the cause A_2 present itself, that event E will accompany it, whether as a consequence of it or not, is q. Moreover, the event E cannot appear in the absence of both the causes, A_1 and A_2. Required the probability of the event E.

The data are—

$$\bar{x} = \bar{a}, \qquad \bar{y} = \bar{b}, \qquad \overline{xz} = \bar{a}\bar{p}, \qquad \overline{yz} = \bar{b}\bar{q},$$

and

$$(1 - x)(1 - y)z = 0,$$

and $\bar{z}$ is required.

$$\text{Now } (1 - x)(1 - y)z = z - xz - yz + xyz,$$

$$\therefore \qquad z = xz + yz - xyz$$

by the last datum;

$$\therefore \quad z < xz + yz - x - yz + 1 \quad . \quad . \quad (1.)$$
$$< xz + yz - y - xz + 1 \quad . \quad . \quad (2.)$$
$$< xz + yz \quad . \quad . \quad . \quad . \quad (3.)$$

$$\therefore \quad \bar{z} < \bar{1} - \bar{a} + \bar{a}\bar{p} \quad . \quad . \quad . \quad (1.)$$
$$< \bar{1} - \bar{b} + \bar{b}\bar{q} \quad . \quad . \quad . \quad (2.)$$
$$< \bar{a}\bar{p} + \bar{b}\bar{q} \quad . \quad . \quad . \quad (3.)$$

Also

$$(1 - x)yz = yz - xyz,$$
$$= yz + z - xz - yz$$

by the last datum;

$$\therefore \quad z = xz + (1 - x)yz;$$
$$\therefore \quad \bar{z} > \bar{a}\bar{p}.$$

Also

$$\bar{z} > \bar{b}\bar{q}.$$

This problem was discussed in the "Philosophical Magazine," by Boole, Wilbraham, and Cayley. Cayley's solution is different, applying to a modification of the problem. Boole goes further, and finds the most probable value of the probability. Wilbraham considers only mathematical probability, and maintains, quite rightly, that we cannot proceed further than above without making assumptions. He says that the disadvantage of Boole's method is, that it does not show whether a problem is determinate. This desideratum is supplied by the method of indeterminate coefficients to which I have referred above.

Monday, 21st April 1879.

Professor MACLAGAN, Vice-President, in the Chair.

The following Communications were read:—

1. The Anatomy of the Northern Beluga (*B. Catodon*) compared with that of other Whales. By Morrison Watson, M.D., F.R.S.E., and Alfred H. Young, M.B., &c., Owens College, Manchester.

(*Abstract.*)

The specimen which formed the subject of this memoir was one of three, imported into England by Mr Farini of London.

The skeleton being already well known, and the state of the parts preventing an examination of the muscular anatomy, attention was directed solely to that of the viscera, of which no complete description had hitherto been given. Drs Barclay and Neill in this country, and subsequently Professor Wyman in America, had previously investigated some points in the anatomy of the soft parts of Beluga, but their descriptions are so fragmentary as to necessitate a more accurate and extended investigation of the viscera.

In addition to a full description of the various organs, a comparison is instituted between these and the corresponding structures of other Cetaceans.

With regard to the relation in which Beluga stands to other genera, the comparative observations detailed in the memoir show that, so far as the soft parts are concerned, Beluga in many respects presents a close resemblance to Grampus and to Globio-cephalus, whilst it differs from both in several minor points. From an examination of the skeleton, Professor Flower[1] concludes that "the Narwhal and the Beluga appear to separate themselves from all the rest, by certain well-marked structural conditions, especially in the characters of the cervical vertebræ. As these two animals are in almost every part of their skeleton nearly identical," Professor Flower is disposed "to unite the two genera into a distinct sub-family, placing it next to the Platanistidæ." Unfortunately, such information as we possess regarding the soft parts of the Narwhal is of too imperfect a character to admit of the comparison being fol-

[1] Trans. Zool. Soc. vol. vi. p. 115.

lowed out. If, however, the number and arrangement of the nasal sacs, as forming an element in the determination of the affinities of different Cetaceans, is deserving of the importance attributed to them by some writers, those of Beluga certainly seem to associate that genus with Monodon, and to separate it from the other genera above named. It should, however, be noted that the sub-division of the trachea into *four* bronchi in Monodon is widely different from that which obtains in Beluga and in every other toothed whale of which we have any knowledge, with the single exception of Pontoporia. In view of the scantiness of information regarding the anatomy of Monodon, the determination of the exact affinities of Beluga must be left to future observers.

2. Fifth Report of the Boulder Committee.

The Committee had submitted to them Notes by the Convener of two visits to the West Highlands (including the Outer Hebrides) which he had made during the summer and autumn of 1878. These Notes, accompanied by diagrams of boulders and striated rocks, afford a large amount of information bearing on the subject of boulder transport, the direction of transport, and the agent of transport.

There has also been laid before the Committee a report by William Jolly, of Inverness, one of its members, "On the Transportation of Rocks found on the Shores of the Moray Firth;" as also Notes by Messrs Somervail and Henderson (Edinburgh), "On Boulders and Striated Rocks in the Pentland Hills."

The Committee have had an opportunity of seeing these Notes and Reports in printed proof sheets. The Convener, on his own responsibility, sent the MSS. to the printer; and the Committee approve of his having taken this course.

Notes by Convener of two Visits to the West Highlands and Hebrides in Summer and Autumn of 1878.

I.—ISLAND OF IONA.

The Convener having occasion to be in this island for a few hours, went to the boulder referred to in the Committee's Second Report, situated on the west side of Dun-Ii hill.

Its peculiar position appearing to him to deserve a more special notice, he gives in fig. 1 a sketch of it taken from the north.

The boulder consists of a coarse-grained granite. But in Iona there is no granite rock of any kind. The prevailing rock is a fine-grained gneiss, approaching in many places to clay slate.

Captain Stewart of Coll was with the Convener when he examined the boulder. On breaking off portions from it, and also from another small boulder lying below, exactly similar in composition, he at once said, "This is Coll granite."

These Iona boulders, in respect both of situation and position, undoubtedly indicate, that they were lodged by some agent which brought them from the N. or N.W. That agent had stranded upon the hill and stuck there till the boulders dropped from it.

From no eastern quarter could the boulders have reached their position. Their site is 250 feet above the sea. The hill on which they are, being 350 feet high, and forming a ridge of about a quarter of a mile running north and south, would preclude access from any eastern point.

The granite in the Ross of Mull, situated to the east of Iona, is different in composition from that of the boulders now referred to. On the east side of Iona there are granite boulders, similar to the Mull granite, as mentioned in the Committee's second Report. But the boulders on the N.W. shoulder of "Dun-Ii" are larger grained and of a different colour; and they occupy a level considerably above most of the granite rocks at the Ross of Mull.

With reference to Captain Stewart's remark, as to the large boulder above referred to being of the same kind of granite as in the island of Coll, the suggestion is so far favoured by the position of Coll, which bears about N.N.W. from Iona, and is distant about 20 miles. But on the other hand, the Convener must state that when he visited the island of Coll a few days afterwards, he found that the *rocks* everywhere were gneiss, and with only occasional veins of granite. The boulders he saw on Coll were of granite.

II.—ISLAND OF TIREE.

1. Heynish Hill, situated near the S.W. end of the island, reaches a height of about 600 feet above the sea-level. This hill consists chiefly of gneiss rock, though in some parts the ingredients become so coarse as to pass into granite.

The hill was ascended from the south side, under the guidance of Mr M'Quarrie, who is tenant of an extensive farm, on which the hill, or the greater part of it, is situated.

The hill on its west side abuts on the sea cliffs. The slope of the hill there has on it a number of rocky knolls.

Almost every knoll has on its N.W. side, facing the sea, boulders, more or less rounded.

The following are the dimensions of some of the larger boulders:—

(1.) $11 \times 8 \times 5 = 440$ cubic feet, resting on the side of a knoll facing W.N.W.

(2.) $9 \times 4 \times 5 = 180$ cubic feet, resting on the side of a knoll facing W. by N., at height of 360 feet above the sea, which is a quarter of a mile distant, and open between S. and N.N.W. This boulder is a coarse granite;—the knoll is gneiss.

(3.) $8 \times 7 \times 5 = 280$ cubic feet, resting on the side of a knoll facing N.W. by N., at height of 365 feet above the sea. Sea is quarter of a mile distant, and access from it is open at any point between S.W. and due north.

(4.) Two clusters of large boulders were met with, the uppermost so placed as to show that it must have come from the westward. The sea is within half a mile to the westward.

On this Heynish hill, the boulders are more numerous on the sides facing the W. and N.W. than on any other side. On the slopes facing the E. and S.E. there are also boulders, but in numbers not nearly so great.

2. After examining Heynish hill, the Convener passed through the island about due north along what is called the Big Cornaig Road. To the eastward of this road there are several rocky knolls, the tops of which are from 80 to 110 feet above the sea. Most of them present bare rock on their west sides, and have boulders also on these sides. One of these knolls was ascended, called "Drumbuim" (meaning yellow rock), for the examination of a boulder observed to be very near its top. Its dimensions were $10 \times 6 \times 6 = 360$ cubic feet. It consisted of a light coloured gneiss;—the rock of the knoll is also gneiss, but dark coloured.

Another rocky knoll, about a mile to the N.E. of the last, was visited to see some boulders, nicknamed, in Gaelic, "The Giant's Pebbles." The legend, as related by a native resident near the

place to the Rev. Mr M'Donald of Helipol, who was the Convener's guide on this occasion, is that three giants living in Barra, wishing to try how far they could throw a stone, took the largest pebbles they could find at Barra, and flung them in the direction of Tiree, which is situated S.E. of Barra, and about 40 miles distant. The story goes, that the stones reached Tiree, and fell very near one another. The knoll referred to is clustered over with huge boulders. Three or four are from 8 to 10 feet high, and from 20 to 25 feet along each side. There may be about 20 or 30 boulders of all sizes; they are on the knoll, and none on the flat ground adjoining, a circumstance suggesting that the knoll, by being above the adjoining surface of the land, had intercepted the agent which was carrying the boulders, and caused them to be deposited there.

3. "*Ben Gott*" hill, on the north side of Tiree, forms a ridge running north and south for about a quarter of a mile, and is from 120 to 130 feet above the sea. A very large number of boulders, chiefly gneiss, are on its N.W. flanks. A few occur on the flat summit, and some are also on the S.E. slope, as if they had been pushed over the top from the N.W. On the flat ground beyond the limits of the hill towards the S.E. there are few or no boulders.

4. In Tiree, the evidence of the sea having stood recently at a higher level is very striking. On a great part of the island there are extensive beds of a stratified muddy sand, sometimes 15 to 20 feet deep, evidently a sea deposit. In other parts of the island there are huge beds or banks of shingle, composed chiefly of well-rounded pebbles of hard gneiss rock, similar to what occurs on the existing shores of all the Hebrides, at places exposed to the action of heavy sea waves. The pebbles in these shingle banks sometimes are twice the size of a man's head, but the great mass of the pebbles are half of this size. They point to a period when the sea must have stood here at least 40 feet, probably more, above the present level, and when, by the force of the waves, fragments of gneiss rock were worn down into elliptic, and sometimes even perfectly spherical, forms. The Convener brought away a few specimens.

III.—ISLAND OF COLL.

1. Under the guidance of the Rev. Mr Fraser, Free Church minister, the Convener visited Bein Hock, a hill on the west side of

the island and very close upon the sea. Its highest point is about 290 feet above the sea.

At the foot of this hill there is another low hill, called Bein Meanach, above 80 feet above the sea. Fig. 2 gives a sketch of both hills. Ben Hock has two boulders on its top, the smaller one, A, 260 feet, the larger one, B, 270 feet above the sea. Enlarged views of these are given in figs. 3 and 4, to show their size and position, and the fact (which Mr Fraser thought curious) that each rests on three smaller boulders. A rests on a rock surface sloping down N.W. at an angle of 16°. The rock on which B rests is nearly flat.

The boulder C on Meanach has nothing peculiar about it, except for size—it being 16 × 20 × 13 feet.

These boulders are a coarse granite, which, however, in some parts passes into a dark-coloured gneiss. The rock of the hill is also gneiss; but they are all veritable erratics, and must have come from some region in the N.W.

2. Mr Fraser next guided the Convener to a spot situated about half-a-mile to the east of Ben Hock, at Grassipol, that he might look at what he (Mr Fraser) considered to be an immense accumulation of boulders.

The Convener, on viewing the place from a distance, thought that the blocks might be only fragments from a cliff adjoining, and not erratics; but, on going to the spot, he found they were boulders, and in positions of much interest. They were lying in many cases over one another on a flat meadow, and formed an elongated heap, more or less parallel with the line of a hill distant thirty or forty yards from them to the S.E. The meadow extended N.E. and S.W. about 350 to 400 yards, and towards the N.W. about 200 yards—viz., in width. The height of the meadow above the sea was about 80 feet. The sea was situated to the N.W., and distant about three-quarters of a mile. The height of the hill above the meadow on the S.E. was about 80 feet. A few boulders were lying scattered on the slope of this hill facing the N.W. It was manifest that the great accumulation of boulders on the meadow along the base of the hill could be best explained by supposing that the boulders had all come from the N.W., and had been stopped by the hill in an easterly movement. One of the boulders

on the meadow was 30 feet in height. (Fig. 5 gives a view of this spot.)

Near the west end of the hill just referred to there was a projecting knoll which had apparently intercepted a number of boulders. There were about twenty altogether piled on one another, and so piled as to indicate that the uppermost could not well have obtained its position except by coming from a N.W. direction. (Fig. 6 is intended to show this cluster of boulders.)

Close to this place there was a vein of quartz, which showed a smooth surface, sloping down towards the N.W., as if polished by some agent which had pressed heavily over it from that direction.

3. In crossing the island, from Arinagour on the east coast to Bein Hock on the west, by the road leading past Arnibost schoolhouse, there is a manifest difference in the size and number of the boulders. At and near Arinagour the boulders are few in number, and small. At and near Arnibost, which is about a mile inland, they become numerous, and occupy significant positions, many being on smoothed rocks facing the west.

At Grassipol, and on the sea-coast adjoining Bein Hock hill, there are boulders of enormous size. The rock on which most of these boulders lie is about 90 feet above the sea, and slopes down towards the W.N.W. at an angle of about 10°. It presents a surface due apparently to some powerful agency which has levelled and smoothed it. Many other examples of this can be seen, close to the highroad near the schoolhouse of Arnibost, and particularly on the low rocky hills south of the road.

These smoothed rock surfaces, sloping down towards the north-west, are easily distinguishable from the natural surfaces of the rock strata. The gneiss rock, especially in this part of the island, is seldom in the form of regular beds. Where such occur, the dip is not towards the N.W., but towards the S. and S.E.

At the S.W. end of the island there are several granite boulders lying on gneiss rocks. One, which was the largest he saw, attracted the Convener's special attention, lying close to the mansion-house of Coll, belonging to Mr Stewart. Its length was 35 feet, its width 15 feet, and its height above the surface of the ground 8 feet. It was leaning on, or at all events pressing against, a mass of gneiss rock on its S.E. side. The granite was coarse-grained

and reddish, because of the felspar in it. Preparations were being made for blasting the boulder. As Captain Stewart was well acquainted with this huge block, he had been probably thinking of it when he saw the Iona Boulder, and compared it to Coll granite.

4. Macculloch, in his account of the geology of Coll, refers to a "block of *augit*" which, he says, he found at a great distance from the shore, and which he thought must "be a *transported* block," as he had seen no such rock *in situ* in the island, and he throws out a conjecture that it may somehow have come from Rum, of which island augit, he says, forms a large portion. This block of augit the Convener did not meet with.

The island of Rum is situated north by east of Coll, and distant about twenty miles.

5. It is somewhat curious that two of these Coll boulders should be described in Dr Johnston's narrative of his tour through the Western Highlands, and in Boswell's Diary. The passages are as follows:—

Johnston says:—"For natural curiosities, I was only shown two great masses of stone, which lie loose upon the ground—one on the top of the hill, and the other at a small distance from the bottom. They certainly were never put into their present position by human strength or skill; and, though an earthquake might have broken off the lower stone and rolled it into the valley, no account can be given of the other which lies on the hill, unless (which I forgot to examine) there be still near it some higher rock from which it might have been torn. All nations have traditions that their ancestors were giants, and these stones are said to have been thrown up and down by a giant and his mistress."

Boswell, in his notes referring to these boulders, says:—"Coll and I passed by a place where there is a very large stone—a vast weight for Ajax. The tradition is, that a giant threw such another stone at his mistress up to the top of the hill at a small distance, and that she, in return, threw this mass down to him—all in sport. *Malo me petit lasciva puella.*"

Again Boswell writes, 9th October 1784:—"As in our present confinement, anything which has even the name of curious was an object of attention, I proposed that Coll should show me the great

stone, mentioned in a former page as having been thrown by a giant to the top of a mountain. Dr Johnston said he would accompany us as far as riding on horseback was practicable—which he did. Coll and I scrambled up the rest. Dr J. placed himself on the ground, with his back against a fragment of rock, while we were employed examining the stone, *which did not repay our trouble in getting to it.* Dr J. amused himself reading a book which he found in the garret of Coll's house."

The stone mentioned in these extracts as at the "top of a mountain," is the one at the top of Ben Hock, marked B, and shown in fig. 4.

The other stone, mentioned as being at a "small distance from the bottom," is C.

Boswell observes that an examination of the boulder at the top of the hill did not repay his trouble in getting to it; but, if he had been able to elicit, from a study of the boulder and its site, the information which geological science now reveals, he would have thought that the trouble of getting to it was well repaid, and he would have been able to give a more probable explanation of how it came to the top of the hill, than that a giant threw it up there at his mistress.

IV.—ISLAND OF STAFFA.

In the Committee's second Report notice is taken of a hasty visit to this trap island by the Convener, which, having occurred on a stormy day, afforded an opportunity of discovering only one or two blocks of red granite.

On account of the interest of finding on an island boulders or even pebbles of rocks, not existing there *in situ*, the Convener, in June last, paid another half-hour's visit to Staffa, by means of the passenger steamboat, which takes tourists to the caves.

He remembered that, on the occasion when he formerly visited the island, the boulders fallen in with were chiefly in the foundations and walls of ruined cottages and sheep stalls. The basaltic rocks of the island were no doubt found less suitable for building purposes. On this occasion, by the advice of the captain of the steamer, the Convener sought for pebbles and boulders in a small bay on the east side of the island. He found several small boulders

lying on the surface, not only of red granite, but also of gneiss, quartzite, and limestone, none of which occur as *rocks* or *strata* in Staffa.

About fifty yards from this place, a bank of consolidated shingle was observed, apparently an old sea beach about 36 feet above high water-mark, from the breaking up of which, in all probability, the boulders above specified were derived.

Dr Macculloch, when he visited Staffa in 1818, noticed these boulders, and was much puzzled to account for them. He says—"I must not quit Staffa without describing a bed of matter which, however foreign to the structure of the island, is by no means foreign to its mineral history, giving rise, at the same time, to geological questions of considerable importance. This is an alluvial deposit, consisting of various *transported stones*, which may be seen on the surface in different parts of the island. It is particularly conspicuous near the landing place, and on the western abrupt edge of the cliff. The fragments are of various kinds—quartz, granite, and blue schist, intermixed with blue quartz rock, and trap—all of them substances which enter into the composition of the neighbouring islands of Rum, Skye, and Mull, but which are found *in situ* no nearer than in the latter island. The distance of Staffa from Mull is not less than seven miles. The surface of the Earth everywhere presents appearances indicating great changes and revolutions, of which none are more unquestionable than the existence of transported stones and alluvial substances in countries far removed from those where similar rocks are now found in their natural situations. The insular position of the example now under consideration, proves that it could not have resulted from the flow of water, whether that flow was gradual or sudden, without at the same time supposing a state of the surface in which Staffa was continuous, at least, with the neighbouring island of Mull." (Vol. ii. p. 22.)

Macculloch here evidently alludes to the theory originally propounded by Sir James Hall for explaining the transport of boulders by a diluvial current. To render such a theory applicable to the Staffa boulders, Macculloch assumes the necessity of joining the island to Mull, though there are now seven miles of sea between them, with a depth of 50 to 60 fathoms. At that time, the idea

of ice, in any form, as a medium of transport had not been thought of.

V.—ISLAND OF BARRA.

This island, near its north end, contains a magnificent boulder. Its size exceeds that of any seen by the Convener in Scotland, and the site it occupies is full of interest. The legend, before referred to, of giants in Barra throwing large boulders to Tiree, may have been suggested to the Tiree people, by hearing that very large boulders existed in Barra.

On figs. 7 and 8, two views of this boulder are given, both from the north. The first view is taken at about 200 yards, the second about 50 yards distance.

The boulder rests on a broad mass of gravel and sand, with numerous cockles in it, at a height above the sea of 230 feet. It is distant from the sea about a quarter of a mile. The present shore is to the north. The great open ocean is chiefly to the N.W., and very partially to the N.E.

The Convener dug below the boulder in several places, and found everywhere sand and fine gravel. A number of rabbit burrows, under and about the boulder, confirmed this observation regarding the materials of the site.

The height of the boulder is about 25 or 26 feet. Its extreme length is from 37 to 38 feet; and its width about 18 feet—assuming two tons for a cubic yard, the weight of the boulder would be nearly 890 tons.

The longer axis of the boulder was found to be N.W. by N.

The flat on which the boulder rests, consists apparently of a sea deposit.* Patches of a similar deposit occur in several spots round and near the boulder, and at higher levels. For example, there is a rocky knoll, about 100 yards to the west, clustered with boulders, 255 feet above the sea. These boulders are lying partly on rock, partly on the shelly gravel. Ben Erival is a hill adjoining the big boulder on the south, and reaching to a height of about 600 feet above the sea. Sand with shells was found among the rocky crevices of the hill, up to a height of 457 feet.

The boulder consists of a coarse gniess almost approaching granite.

* See note on page 67.

The gniess of Ben Erival, and of the other adjoining rocky knolls, is more close-grained in composition.

On fig. 9 there is a ground plan, from memory, showing the position of the boulder in relation to adjoining hills. Ben More, which reaches a height of 330 feet above the sea, and is about a mile to the north, is covered with thick beds of sand and fine gravel, full of cockle and other sea shells.

It is also worthy of notice that at present, the bay, immediately to the north of Ben Erival, has in it an immense bed of living cockles—so immense that it is found profitable to gather them from time to time, and send them to Glasgow for sale.

There is something therefore in the sea or the sea-bottom in this district, which now as formerly favours the growth of the *Cardium edule.*

That this "Big Rock of the Glen" forms a veritable boulder, and that, when it was brought to the spot which it now occupies, it was deposited on what was then a submarine bank, not much doubt can be entertained.* The boulder must therefore have been floated to the spot where it now lies—but from what quarter? From the S., S.E., or S.W., come it could not;—as Ben Erival, on whose north flank it rests, and which ranges for about two miles east and west, precludes that idea. There being open sea to the N.W. and N.E., from either of these quarters it might have come, but from no other.

The plan on fig. 9 explains more clearly how the boulder might have been floated from these quarters, and been intercepted in its further progress to the south by Ben Erival.

An examination of the numerous smaller boulders in this district, also indicated transportation from some point between west and north. The following are cases:—

1. To the west of the big boulder, and about 100 yards distant, there is a small but steep rocky knoll (fig. 9, letter *b*) whose top reaches to a height of 255 feet above the sea, and which is covered with boulders, especially on the N.W.

On a minute study of the relative positions of the boulders on this

* The submarine character of the bank does not depend solely on the presence in it of sea-shells, for they might have been blown up from the existing sea-shore by storms. But the materials forming the bank being found, by digging under the boulder, to consist of sand and gravel, they afford the strongest evidence of a submarine origin.

knoll, it was found that those which were uppermost must have come from the N.W., otherwise they could not have got into the positions they occupy.

There were no boulders near the top of the knoll on the S.E. side ; but at the base of the knoll on that side, several boulders were lying, which might have fallen from the top. They were not heaped on one another, as they were at the top of the knoll, but lying separate.

2. About 200 yards to the N.E. of the big boulder there is a boulder on smoothed rock which dips due north at an angle of 20°. The size of the boulder is 5 × 4 × 4 feet. The steepness of the rock surface on which it lies, is so great, that it would have a better chance of obtaining and retaining its position by coming from the north, than from any other quarter.

3. About 300 yards to the S.E. of the "big boulder" there is a boulder $8\frac{1}{2} \times 6 \times 5$ feet, at a height of about 228 feet above the sea, shown on fig. 10. The boulder at its east end presses closely on a rock, which has prevented it moving further in an easterly direction.

4. On the N.W. side of Ben Erival, where its sides slope down steeply to the sea, there are numerous boulders, and many of them pressing in like manner against the rocks of the hill, in such a way as to show that they must have come from some point between west and north. They are at various heights, from 400 to 500 feet above the sea.

5. There is a low hill to the N.N.W. of Ben Erival, adjoining "*Traigh Vore*," or Great Strand (a narrow neck of sand which here separates the east and west shores), through part of which an open fissure in the solid rocks runs for some distance. It has evidently been one of those rents alluded to by Macculloch in his Account of Barra, which had once been filled by trap, but "of which the exposed portions have been washed out." (Vol. i. p. 89.)

The height above the sea-level is about 120 feet.

For about 80 yards, this rent or fissure now presents two vertical walls of gneiss, from 11 to 12 yards apart, and from 8 to 14 feet high.

The direction of the rent is (by compass) N.W. and S.E. The rocks on the *north* wall are rounded, and in many places present

smoothed surfaces. The rocks on the *south* wall are rough and jagged. The appearances on the north walls can be naturally accounted for by the action of a strong sea current moving from W.N.W., which would, with any bodies floating in or swept along by it, grate against the north, but not against the south wall. (See fig. 11.)

6. Ben More is a hill on the farm of Eoligarry tenanted by Dr MacGillivray. Its west end forms a steepish sea cliff, rising up to a height of 330 feet above the sea. Half way up this sea cliff, there is a boulder, 20 × 10 × 5 feet, resting on the rocky surface, which here dips towards the W.S.W. But the rock, judging by the marks on it, has been smoothed by something passing over it from the N.W., and the boulder is blocked at its S.E. end by a vertical portion of the hill, as shown on fig. 12.

7. At Castle Bay, which is at the south end of Barra, the hills are seen to be more covered with boulders on their N.W. sides than on any other. This observation, however, was made only from the steamboat.

Mr J. F. Campbell, in his paper on the "Glacial Phenomena of the Hebrides," states that, in Sept. 1871, he took rubbings of striæ at Castle Bay, showing that the striating agent had come from N. by W. (magn.)

He mentions also that on the small island of Bernera, above 12 miles to the south of Barra, "the last of the Hebrides," he got striæ at a height of 720 feet above the sea, crossing the strike of the rock, from N.N.W. (*Lond. Geol. Soc. Journal*, vol. xxix.)

In coasting along the east shore of Barra it is perceivable, from the deck of the steamboat, that the rocks on the sea cliffs which face the N.W. have been smoothed, whilst the rocks facing the east are rough and jagged.

8. On the hill called Scurrival, whose west side rises abruptly up from the sea to a height of about 240 feet, the hard gneiss rocks present many proofs of grinding, and also of transporting agency from the N.W.

The rock-strata here are tolerably horizontal and form blocks lying about north and south. The vertical sides facing the sea present frequent smoothings, which could have been made by the action of a strong N.W. current, especially if loaded with

ice. (See fig. 13.) The surfaces facing the east present no smoothings.

The examples are numerous on this hill of boulders blocked on their S.E. ends or sides. They are cases exactly similar to that shown on fig. 12. These boulders are within 200 yards of the open ocean, and less than 100 feet above its level. The situation and position of these boulders combine to show that they *must* have come from the westward—though in that direction there is only the wide Atlantic.

At the very top of the hill, which consists of well rounded and smoothed surfaces of gneiss, numerous boulders lie scattered—most of them on that part of the top facing W.N.W.

VI.—ISLAND OF SOUTH UIST.

1. Beginning near the south end, notice has to be taken of a well striated gneiss rock, recently exposed by the removal of materials for the high road. The spot is on the east bank of Loch Dunkellie and at the west side of a hill called Carshavaule, which is marked on the Admiralty map as 226 feet high. The striated rock is only about 20 feet above the sea-level.

The rock had been covered by a bed of coarse sand intermixed with clay, so that its surface had been protected from the weather. The protecting cover contained numerous pebbles, hard and angular, the pressure of which on the rock, if they passed over it, would probably cause striæ.

The rock consists of strata which dip W.S.W. at an angle of about 10°. They were thus conveniently situated for being struck and pressed on by any striating agent from the west.

The lengths of the blocks rounded and striated were respectively, 4, 7, and 5 feet.

The striæ run in a direction N.W. by N. and slope up towards S.E. by S.

If these striæ were caused by rough stones carried in a strong current flowing from the N.W., or pushed by floating ice, the striæ would slope upward in the above direction, because the current would in this low lying spot have to rise, to pass through a valley situated close at hand, immediately to the south of Carshavaule hill.

Mr J. F. Campbell in his paper (before referred to) states that in

a quarry by the roadside of Boisdale in South Uist, he observed "striæ running from N. 40° W. (magn.) pointing at a gap in the hills." This is probably the same spot as that noticed by the Convener. It was shown to him by Mr Drever, factor to Mrs Gordon of Cluny.

2. Loch Boisdale, a sea loch, is situated on the east coast. On the north side of the loch, there is a hill called Kennet, reaching to a height of about 890 feet.

The rocks on its N.W. side, from bottom to top, present numerous examples of flattened and rounded surfaces. The surfaces facing the S.E. on all sides of the hill are rough and angular. On the west side of the hill, at various levels between the bottom and the top, there are numerous boulders, some of them, by the way in which they lie, affording unmistakable evidence of the direction from which they came.

For example, there are two boulders on a narrow shelf of rock which slopes down S.W. at an angle of 40°. The shelf is 96 feet above the sea, and quite close to the sea. The shelf is on the sea cliff, which is so steep, that the wonder is, how the boulders could have found a cleft in it to hold them. (Fig. No. 14 shows these boulders.) On the east side of the boulders there is a projecting ledge, against which the eastmost boulder (A) presses, and which had stopped its farther progress eastward. Another boulder (B) lies upon (A), and which, to get on the top of (A), must have come from some westerly point,—probably the N.W. A line through the chief points of contact and the centres of bulk runs in a direction N.N.W. A study of the boulders on the spot showed that, if they had been brought to this site from any other direction, they would inevitably have slid down the steep rocky bank into the sea. These blocks are nearly equal in size, viz., about 5 × 3 × 2 feet.

Fig. 15 shows a large boulder of coarse granite resting on a wedge of gneiss rock. The wedge or knob is under the boulder at its east end, and tilts up the boulder slightly so as to show daylight under the boulder at that end. It rests on the ground chiefly at its west end. By this wedge (*a* in the figure) the boulder has evidently been stopped in its progress from the N.W. From its rounded shape, one might infer that the boulder had been rolled or

pushed for some distance before it was stopped. The west side is much rounder and smoother than any other side; so, probably after it had stuck, the current which brought it, beat and chafed on its west side, and smoothed it. This boulder lies on a level plateau of rock about 202 feet above the sea. It is all open country towards the N.W. and N.E., whilst the Kennet hill, reaching to a height of 890 feet, is within half-a-mile of the boulder to the S.E. and E.S.E.

On the west slope of this hill, at a height of 300 feet above the sea, the gneiss presents a rocky surface sloping down towards the west at an angle of about 10°. A boulder of coarse granite, $7 \times 6 \times 4$ feet, rests partly on it and on another smaller boulder underneath. This boulder, at its S.E. end, abuts against the rock. It has come, therefore, almost certainly, from some north-westerly point and stuck there. (Fig. 16 represents this case.)

Not far from the top of the hill, viz., at 712 feet above the sea-level, there is a very large angular boulder on a flat ledge of rock, on the N.W. side, with open country in that direction. This boulder is $19 \times 13 \times 8$ feet. Its further progress eastward has evidently been stopped by a projecting cliff of the hill on its south-east side, as shown in fig. 17.

3. Several large boulders may be seen at a small village, where the Free Church and Roman Catholic Church are situated at a junction of the roads from Barra and Loch Boisdale, about two miles to the south of Askernish. There is here a whole cluster of boulders. One, $16 \times 6 \times 5$ feet, leans slanting upon the others, and must have come from the N.W. to attain its position.

4. On the hill to the east of Askernish, and on its side facing the west, there is a surface of rock, sloping down W.S.W. at an angle of 30°, well smoothed. A boulder rests on this slope, partly on the surface of the rock and partly on some smaller boulders which lie between the rock and it, near its S.E. end. The boulder has evidently obtained its position by coming from the N.W.

This is more clearly proved by a number of ruts or striæ, visible on the rock a few feet below the boulder, which run, as shown on fig. 18, by the arrows, in a direction from N.W. to S.E. That the striating agent first struck the rock from the N.W., is made evident by the circumstance that most of the striæ are deeper and wider at

their N.W. than at their S.E. ends. This change in the striæ can be accounted for by supposing that the striating agent as it moved over the rock, acted with a lessening pressure, by having rebounded from the rock after the first impact.

5. On Mingary Hill, reaching a height of about 600 feet above the sea, three miles N.E. of Askernish, many boulders occur, especially, as usual, on the N.W. flanks. Most of them occupy separate spots—but in some places they are in clusters—heaped on one another. In this last class of cases, there is generally a knoll of some kind standing up above the general surface, on or round which the boulders lie.

One of the most interesting spots on this hill is a spur from it projecting N.W., to which Mr Drever (who resides at Askernish), conducted the Convener. His object was to point out there a boulder of considerable size which had shortly before been seen by Mr Jolly of Inverness. The hill in question is shown in fig. 19. The hill here reaches to a height of 270 feet above the sea, and it slopes down at an angle of about 15° to the N.W. But about 30 or 35 feet from the top, there is a horizontal plateau, on which a number of boulders lie together. Has this been an old sea-margin, from which the smaller stones have been washed away, leaving on it, as on a beach, the heavier boulders? The largest boulder in the figure, lower than all the rest, is 11 × 9 × 8 feet. It lies on bare rock sloping down towards the N.W., from which quarter it, as well as all the others, had apparently come. The transporting agent seems to have struck upon the hill, and discharged its cargo there.

Very near the top of the hill, there is a rocky surface, rounded and striated, the striæ running N.W. by N. A vein of quartz about 3 inches wide crosses this rock, and for about 12 inches it presents a beautifully smoothed surface.

6. At a place called Jocdar, situated on the main road one and a half mile south of the Ferry between Uist and Benbecula, smoothed rocks have been exposed to view by the removal of gravel, &c. These rocks are at a height of about 25 feet above the sea. The rocks are literally covered by parallel striæ, ruts, and grooves, the direction of all which is N.W. by W.

On these rocks there are twelve or fourteen deep ruts and

grooves, some of them 4 or 5 feet in length. One of them, at its N.W. end, measures 8 inches across, and 2 inches in depth; another measures at its N.W. end, 12 inches in width, and $1\frac{1}{2}$ inch in depth; another 9 inches in width, and $1\frac{1}{4}$ inch in depth. These, and most of the others, show a greater depth and width at their N.W. than at their S.E. ends. In fact, they all gradually lessen and disappear towards the S.E.

At this place, the smoothed faces of the rock slope at an angle of 10° or 12° to the westward.

7. There is another exposure of well rounded, smoothed, and striated rocks, close to the Ferry between Benbecula and Uist—*i.e.*, about half a mile to the west, on the south side of a bye road. The rocks are here, as at the place last mentioned, of hard gneiss, and most beautifully polished. They had been covered by a bed of clay containing numerous hard pebbles, a portion of the bed still remaining upon the polished rock. Here, as at Jocar, some of the grooves are several inches in width, and as much as 2 inches in depth, and several feet long. The deepest and widest ends are also, as before, at the N.W.

One of the rounded rocky bosses is polished not only on the top but at the sides, as shown on fig. 20. Having regard to the bearings of the knoll, which is elliptic in shape, the polishing and striation on both sides could have been effected only by a current flowing from the N.W.*

8. On the road between Grogarry (the mansion-house of Mrs Gordon of Cluny) and Loch Skiport (on the east coast), the following places of interest were observed:—

At about $1\frac{1}{2}$ mile from Grogarry, on the south side of the road, the hard gneiss rock, which had recently been uncovered, was found to have been ground down and polished into extensive surfaces dipping N.N.W., at an angle of about 20°. These surfaces were covered by innumerable striæ, and by several ruts and grooves—all running in a direction E.S.E. up the face of the rock at an angle of 7° or 8°. It is very probable that a current from the N.W., loaded

* These two beautiful examples of rocks, smoothed and striated, at Jocar and at the Ferry, were pointed out by Alexander Carmichael, Esq., Creagorry, who resides near the Ferry. Both he and Mrs Carmichael took much interest in the Convener's researches, the latter kindly giving to him sketches which she had made of several interesting boulders.

with hard gritty materials coming against the rock dipping as above explained, would be deflected in its course along the face of the rock from S.E. to E.S.E. At the N.W. end one groove measured 2 inches wide and $\frac{1}{4}$ inch deep; another, 2 inches wide and $\frac{1}{8}$th inch deep. They became fainter towards their S.E. ends.

At another place on the road side, the striæ ran W.S.W., but the surface of the striated rock faced the south, and it was in a confined valley only about 30 feet above the sea.

On the hill adjoining, 122 feet above the sea, a granite knoll on an open moor showed a deep rut about 18 inches long, running from W.N.W., its west end being deepest and widest.

At another place the boulder had in its progress eastward been intercepted by a vertical ledge of rock at its east end, and it was resting on a horizontal bed of rock, just as in figs. 10 and 12.

At another place there were 5 or 6 huge boulders piled over one another, all resting on a rocky knoll, standing above the general surface of the adjoining district. The topmost boulder, lying in a slanting position on the others, could have obtained that position only by coming from the westward. This spot was 80 feet above the sea.

About 3 miles to the north of Askernish, on the east side of the main road, there is a perched block of granite, on the pointed summit of a rocky hill about 130 feet above the sea. Two views are given in figs. 21 and 22. The base on which the boulder stands is exceedingly narrow. The boulder is in size 14 × 12 × 8 feet, and its contact with the rock is only 6 × 4 feet. A steep hill rises near the boulder on its east side, but the boulder could not have fallen from it. That hill would arrest an iceberg or ice-floe, if the boulder came in that way from the west. As the ice melted, the boulder might have subsided gently on the peak. Some smaller boulders cap a rocky knoll below, as shown on the figures. All these indicate transport from the N.W. by some means.

9. *Loch Eport* is a remarkably narrow arm of the sea, on the east coast, which runs more than half-way across North Uist, towards the west coast. From the deck of the steamboat numerous boulders were seen, most of them resting on knolls. The smooth faces of the rocks were all strikingly towards the N.W., whilst the rough and jagged rocks all fronted the S.E.

(10.) *Loch Maddy*, a sea loch on the east side of North Uist. A walk for about a mile among the hills, during an hour that the steamboat was discharging cargo, showed that the rocks had their smoothest sides towards the N.W., and their rough sides towards the S.E. Boulders in great numbers were lying on these smoothed surfaces, and on the N.W. sides of the hills.

Before concluding his notice of Uist, the Convener may advert to one feature in the physical aspect of the island, viz., the extraordinary number of small lakes. When any of the hills are climbed, which afford even a tolerable view of the low grounds, it would almost seem that more of the island consists of lakes than of dry land. The cause of this feature probably is, that the general level of the island is so little above the sea, that the hollows occupied by these lakes can never be emptied. It is another striking feature, that most of these hollows lie in the same direction, viz., W.N.W. and E.S.E.

VII.—ISLAND OF CANNA.

The Convener when in the steamboat made the acquaintance of Mr William Bain, generally residing at Tiree, who takes contracts for erecting buildings in the Hebrides. He mentioned that he had lately built a new schoolhouse in Canna, an island situated near Rum. He told the Convener that he had found on the islet of Sanda, which forms the south side of Canna Harbour, blocks of a red sandstone which he made use of for the lintels and corners of the school doors and windows. The largest of these blocks was about $6 \times 4 \times 2$ feet. He knew that these sandstone blocks differed from the rock of the island, which he described as a sort of blue slaty schist, ill-adapted for building. He recognised these red sandstone blocks as of the same nature as rocks in the island of Rum, which were good for building purposes, as he had quarried them for that purpose.

This statement by Mr Bain is confirmed by Macculloch. He says—" Sandy isle, like Canna, presents examples of a circumstance rare in the Western Islands, viz., loose fragments of a different rock from that of which it is formed, lying on the surface. There are large blocks of red sandstone somewhat rounded, and they are found in considerable abundance on the flat shores of both. 'The

rock of which they consist is that which forms so large a portion of Rum and of Skye.'" (Vol. i. p. 467.)

With reference to the conjecture that these red sandstone boulders in Sandy may have been transported from Rum or Skye, a probability of its correctness is afforded by the circumstance that the red sandstone rocks of Rum and Skye are situated on the sides of these islands facing Sandy and Canna.

VIII.—HARRIS.

1. At Rodel, the south end of Harris, there is a hill called Strondaval, 638 feet high. It is steep and rocky on all sides, especially the west and south. The Convener, under the guidance of Lord Dunmore's gamekeeper, scrambled along its south and east sides, and found that the smooth faces of the rocks all looked towards the W.N.W. On the east side of the hill there was an entire absence of smoothed rocks. That side had apparently been the lee side, not having been grated upon by the agency, whatever that was, which had smoothed the west side.

There were many boulders on the hill, chiefly angular; some pretty large, but none of any special interest.

2. At Borve, on the west coast, about half-way between Rodel and Tarbert, there is a remarkable accumulation of boulders on the side of the hill, sloping down to the sea. The general dip of the hill (which reaches a height of about 800 feet) is towards the west or west by north (magn.). The rocks are of gneiss, and present a series of beds, layers, or benches more or less horizontal, forming, as it were, a gigantic staircase along the hill face for about half a mile, several hundred feet high—all more or less covered by boulders. These benches of rock, in many places, show that they have been rounded by severe pressure from west by north. The boulders which lie on them give evidence of transport from the west.

Fig. 23 is intended, by a sectional view of the hill, to show the disposition of its rocks and the position of the boulders on them.

Fig. 24 gives a view of two boulders lying on a portion of the rocks forming the hill just mentioned. The position of both indicates blockage and stoppage on their east sides. Their own relative positions afford similar evidence.

3. Near Lach Castle valley, *i.e.*, about $1\frac{1}{2}$ mile south of it, and about 2 miles north of Borve, a striated rock was observed on the roadside. It had recently been uncovered by the removal of road materials. The rock was Silurian. It was well smoothed, and sloped gently to the west. The striæ were minute, but quite discernible, and running N.W. The rock was on the N.W. side of the hill called in the Admiralty chart Carron Hill, and close to the sea, which was all open toward the N.W. As Carron Hill, with a height of 786 feet, was to the S. and S.E., the presumption afforded by the surrounding land features was that the striating agent had come from the north.

4. Lach Castle bay and valley is shown on figs. 25 and 26. When the tide is out, the road between Borve and Tarbert crosses a sandy flat; but when the tide is up, the margin of the land is indicated by the dotted line. There is an immense accumulation of boulders on the S.E. side of the hill marked A, where Carron Hill, above referred to, is situated. The X on the fig. indicates the spot where the striated rock was observed.

If, when the sea stood say 1000 feet or more above its present level, boulders were brought by a current from the N.W., the facts observable in this Lach Castle valley could be explained.

In that case, the current would flow through the valley, pressing most upon the range of hills on the east side, and smoothing its rocks; whilst the rocks on the west side of the valley would remain rough. This is found to be the case on an examination of the two sides of the valley.

Icebergs or floe ice carrying boulders may have flowed up the valley from the north, discharging them chiefly on the hills along the east side of the valley. These hills bear on their sides and ridges numerous boulders, some of large size. Several of these were examined, and one or two gave indubitable proof, by their sites and by their own positions, that they had come from the north or N.W.

In the centre of the valley there is an elongated ridge (as shown on fig. 25, *l.c.*) which bears far more boulders than the depressed portions between it and the sides of the valley. There may be two ways of accounting for this. If the valley was originally of its present form, any ice borne on a current flowing through the valley

from the N.W. would strand more frequently on the central ridge and on the east side than elsewhere. If the valley was originally filled up to the level of the central ridge, the debris at its two sides must have been scoured out by the rivers now flowing through it; and in this case, whilst the boulders in these parts would gradually find their way to the channel of the rivers and to the sea, the central ridge would retain most of the boulders originally lodged on it.

On this ridge the smoothed rocks face due N. and not N.W. This deviation may be accounted for by the valley here being between two elevated ranges of hills running almost due north and south, which would cause the current to flow in a direction due south.

One of the boulders on this central ridge measured $16 \times 14 \times 12$ feet, = about 200 tons in weight.

It will be observed that on the S.E. side of A there is a large accumulation of boulders. These might have been floated there by an eddy occasioned by the projecting headland near A.

5. Almost $1\frac{1}{2}$ mile to the south of Tarbert, there are several large boulders, on the east side of the high road leading from Tarbert to Lach Castle. The Convener, on examining them, found them to be granite of a grey colour, whilst the rocks in the hills about them are gneiss.

These boulders being within half a mile of the sea, which is to the eastward, and being at a height of about 100 feet above the sea-level, it might have been presumed that they could have come from the eastward. But these boulders were on hill slopes facing the west; and as the slopes were steepish, it was not easy to understand why, if the boulders had come from the east, they had not rolled to the foot of the slopes. On the other hand, there were towards the west and north, ranges of hills, reaching to heights above the level of the boulders, viz., to about 200 or 300 feet. But towards the N.W., and at a distance of three-quarters of a mile, there was a gap or depression in the hill range; and, on applying the spirit-level, it was found that the depression was about the same level as the boulders, so that they might have come from that quarter by flotation, and been lodged on their present sites.

Fig. 27 is intended to represent what has just been described. B are the boulders, AAA a range of hills to the westward, with a gap in those at G, bearing N.W. from the boulders.

6. On the hills north of Tarbert there are many unmistakable signs of a N.W. current up to the highest level which the Convener was able to climb to, viz., 800 feet above the sea.

(*a.*) There are multitudes of knolls or bosses of rock, rounded and smoothed on their west sides, but rough on their east sides. There are none which show opposite markings.

(*b.*) There are many cases of boulders lying in such a way as to show that they had been stopped there in their progress eastward. One example is given in fig 28, where hard gneiss rocks had been rounded and smoothed from the westward, and a number of boulders —several of granite—were lying at the base of these rocks. A westerly current, if it smoothed the rocks, might have also brought the boulders.

(*c.*) At Avon Sue, or Fincastle, the handsome mansion-house of Mr Scott, banker, London, on the sea-shore about 11 miles west from Tarbert, the following observations were made:—

A little way up the hill, above the stables, a striated rock was met with. The smoothed rock sloped down towards the sea in a direction S.S.E. Three ruts on this smoothed surface when measured were found to be from 18 to 23 inches long, and about 2 inches wide Their direction was due east and west. The ruts were deepest and widest at the west end. In consequence of the direction in which the smoothed rock sloped, a N.W. current, coming against it, would be diverted into a direction nearly due east. The lines of the ruts in that direction ran up on the rock surface at an angle of about 8° or 10°.

On this hill slope there were several boulders whose position indicated clearly that they had come from the westward,—that is, from the sea. These proofs were the same as those explained in regard to other cases (see figs. 10 and 12), and therefore need not be repeated here.

IX.—ROAD FROM TARBERT TO STORNOWAY.

1. Where the road leaves the sea and strikes north there are enormous boulders, partly buried in drift, on the west flanks of the hills. This road reaches its summit level at about 650 feet —a distance of about 2 miles. The valley is narrow, between ranges of high hills on each side, and runs in a direction E.N.E.

As the summit was approached, it was observed that the boulders became less in size and fewer in number. This is quite intelligible if all the country had been under the sea, and a current flowing from the W.N.W., as this valley, on account of its direction, would have no great force of current in it, and the passage would be too narrow for much ice to pass through it.

At the summit level, a striated rock was observed, the striæ running W.S.W., *i.e.*, parallel with the general axis of the valley.

2. At *Ardvourlie* there is a *trainée* of boulders extending for at least half a mile, running in a direction east by north. On examining several clusters of boulders, it became apparent that the boulders had come not from the east but from the west.

Ardvourlie, to which this *trainée* reached, is close to the sea, viz., on a branch of Loch Seaforth, and the valley rises in a direction about west by south. In following with the eye the line of the *trainée*, it was seen to point towards a gap or depression in the range of hills at the west, distant about two miles. The Convener regretted very much that it was not in his power to follow this *trainée* and investigate the correctness of his conjecture—that the boulders may have come from the westward through the gap.

3. *Soval* is a shooting lodge of Sir James Matheson, on the road to Stornoway, and about 12 miles from it. To the east of Soval there is a rocky ridge, distant about half a mile, and at a height of about 220 feet above the sea.

On this rocky ridge the smooth faces of the rocks look towards the N.W. Indeed, in the whole of the district north of Ardvourlie, a distance of about 15 miles, this was the case with all the hills passed.

On the ridge just mentioned there was a boulder close upon its edge, which gave clear indication of a N.W. current. The rock forming the site of the boulder had been smoothed, and it sloped towards W.N.W. at an angle of from 20° to 25°. The boulder is in size $5\frac{1}{2} \times 3\frac{1}{2} \times 2$ ft. The longer axis of the boulder is W.N.W., and its sharpest end is towards the west. It is shown in fig. 29.

4. For 7 or 8 miles to the south of Stornoway, the district passed through by the high road from Tarbert, Ardvourlie, and Soval, consists of an extended plain covered by peat and coarse pasture. The height above the sea is from 200 to 230 feet. No hills or even

rocks are visible. There is an entire absence of boulders. From the banks of the small streams and the ditches by the side of the road, it was plain that sand and gravel lies in great beds immediately below the surface.

X.—NORTH PART OF THE LEWIS.

The Convener, through the courtesy of Mr M'Kay of Stornoway, Sir James Matheson's factor, was enabled to visit Lochs Ourn and Sheil, arms of the sea, to the south of Stornoway, on the east coast of Lewis. He landed from the steam yacht at both of these places, and had time to ascend several hills.

The rocks here, as at most other places, present their smooth faces to the W.N.W., their rough faces to the E.S.E.

At Loch Ourn, one of the boulders at a height of 200 feet above the sea (size 7 × 5 × 4 feet) lay on the west side of the hill upon a rock surface sloping down to N.W. at an angle of 20°.

At Loch Sheil, at a height of 325 feet above the sea, the only boulder of any size (10 × 6 × 4 feet) was on a hill-side facing W.N.W., and on a rock surface sloping down in that direction at an angle of 15°; but 5 or 6 yards below the boulder, the slope down of the rock was 30°. The longer axis of the boulder pointed west by north.

The yacht steamed round the "Shiant" Islands, to afford an opportunity of seeing their magnificent basaltic columns. They are on a grander scale than those in Staffa, and exhibit remarkable curvatures. These islands are partly composed also of schists and stratified rocks, more susceptible of diluvial action than the hard basalt; and it was easy to see even from the deck of the steamer that a N.W. current had acted on them. Boulders also of considerable size were observed on the slopes facing the N.W.

The Convener regretted much that there was no opportunity of landing.

4. *Uig*, on the west coast of Lewis. On the hill near the parish church, about 186 feet above the sea, all the smoothed rocks front W.S.W., and on many rock surfaces sloping down towards west boulders were lying.

At two places, rocks were found with ruts and striæ. As at both, the general features were the same, one only may be illustrated by

a diagram, fig. 30, and on account of a peculiarity that the ruts crossed a fissure in the rocky surface.

The general surface of the smoothed rock at both places dipped due west at an angle of 12°, and looked out on the Atlantic Ocean, which was only a quarter of a mile distant. The direction of the ruts and striæ was the same at both places, viz., W.N.W., and rising up E.S.E. on the surface of the rock at an angle of about 10°. Probably owing to the obstruction which a current striking the rocky surface, dipping due west, would meet, a W.N.W. direction would be the result of a current from the N.W. At both places the ruts were wider and deeper at the west ends than at the east ends. One of the ruts was carefully measured, and showed at the west end a width of 2 inches and a depth of $\frac{3}{4}$ of an inch; at the east end a depth of $\frac{1}{8}$th of an inch; and there, the width ceased to be distinguishable.

The peculiarity before referred to was a small fault or fissure crossing the rocky surface as shown on the figure by the letters *a*, *b*, *c*. The fissure had caused, as it were, an upthrow of the rock, of about $\frac{3}{4}$ of an inch. Where the rut crossed the fissure, there was a slight deviation in the line of the rut, as shown in the figure. The hard pebble or stone which produced the rut, meeting with the obstruction caused by the upthrow, had been slightly diverted from its course, but it had eventually passed over the upthrow, breaking off the edge of the rock.

5. *Miavig* is a small hamlet situated on an arm of the sea, branching up from Loch Roag on the west coast of Lewis. About half a mile to the N.W. of Miavig a hill called "Dramamin Voltas" (height above sea 270 feet) rises above the general surface of the district, and has been the means of arresting a multitude of large boulders. They are clustered and piled over one another upon the north and west sides of the hill (see fig. 31). A few lie on the east side, a little way below, as if they had tumbled or slipped down from the top.

6. On the road from "Garry-na-hine" to Loch Carlowrie there are several objects of interest.

The hills are rocky. Their smoothed faces are all, as elsewhere, on and towards the west; their rough faces on and towards the east. There seems, however, to have been a slight change here in the direction of the current; for whilst at Breasdeit village the smoothed

rocks faced W.N.W., towards the north there was a gradual change to due west, and then ultimately at Carlowrie to W.S.W. and S.W. These deviations from the normal direction occur at low levels. Near the hill tops, at from 300 to 400 feet above the sea, there was little deviation from W.N.W.

The Convener examined a striated rock near the north end of Loch-na-Muilve mentioned by Mr James Geikie in his paper on the glacial phenomena of the Hebrides ("London Geological Society's Journal" for 1873, p. 537). As there are some points of interest on this rock not included in Mr Geikie's notice of it, a representation of the rock is given in fig. 32.

The rock dips down towards W.S.W. at an angle of about 30°. There are two portions of smoothed rock visible as shown in the figure—the space between them consisting of a stony clay, which probably lies on rock, though the rock is not visible. The part of the rock which is visible has evidently been smoothed by the passage over it of some material—such as the clay, of which a portion remains, containing pebbles and stones. The striæ and ruts are not all parallel. The lowest rise upwards across the rock at an angle of about 8°. The ruts in the upper portions of the rock surface rise up more quickly till at length, in the highest part, they rise at an angle of about 26°. Another feature is, that some of the ruts are deeper and wider at their west end than at their east end. The directions of the lowest ruts is N.W., of the highest W.N.W. If the general line of the current was W.N.W., the highest ruts would be more likely to indicate that direction than the lowest.

At Garry-na-hine, and also on the hills about two miles north of it, there are numerous cases of boulders on smoothed rock surfaces facing the west, the boulders being blocked at their S.E. ends by special obstructions, which were in each case distinctly observable.

7. Mr James Geikie refers to a water shed called "Beinn à Bhuna" on the road between Stornoway and "Garry-na-hine," where he says there are "smoothed and glistening domes of gneiss."

The Convener examined all the rocky knolls at the place referred to, on both sides of the summit level, which is about 400 feet above the sea. The smoothed surfaces are numerous, and particularly on the west side, where they face the N.W. The boulders are also more numerous on that side, and are generally on rock surfaces

dipping toward W.N.W. at angle of 10° or 12°. The longer axis of the boulders was mostly in the same direction.

8. About two miles east of "Garry-na-hine," a quarry on the road side at a height of about 160 feet above the sea had been opened for road materials. A tough strong clay covers the gneiss rocks here; and above the clay there are beds of gravel and sand, all evidently sea deposits.

9 The Convener visited the rocking stone on a hill 358 feet above the sea near Tolsta, about 12 miles to the N.E. of Stornoway. Resting on the gneiss rock, at a part of its base near the centre, it can be moved a few inches up and down by the hand only. It is about 18 feet long, 5 feet high, and 4 feet wide. Its longer axis points N.N.W. There is an opening among the hills in that direction, through which it might have been floated to its site; whilst towards the S.E. the hills reach to a greater height, and would prevent the boulder coming from that quarter. The boulder is extremely angular, and has undergone no rolling or pushing.

10. About five miles to the N.E. of Stornoway there are three hills called the Barvas Hills, each from 800 to 900 feet high.

The Convener examined the two eastmost hills, and found as follows:—

Both hills on the N. and especially the N.W. sides, present precipitous cliffs, and surfaces well rounded and smoothed; but no striæ were seen.

On the W. and S.W. sides of the middle hill, there are also a few smoothed rocks.

There are boulders on both hills on all sides, and up to nearly the top, but they are in greatest numbers on the N.W. sides.

On the middle hill, very near the top on its N.W. side, one of the smoothed rocks is traversed by a thick vein of quartz. The quartz also presented a smoothed surface. A specimen of it was brought away.

There was one boulder (6 × 5 × 4 feet) lying on a side of the middle hill facing N. by E. It might have come from the N.W., as in that direction there was no obstruction. From N.E., E., S.E., or S., it is difficult to suppose it could have come, on account of the interposition of the eastmost hill.

On the eastmost hill, at a height of 700 feet on the north side,

rocks were found smoothed from the N.W. A portion of smoothed quartz was found here also.

11. The Convener drove along the coast from Barvas village to Dalbeag, a distance of about 9 miles. He was unable to reach Dalbeag hills, about 2 miles farther on. He could see, however, that these hills presented large surfaces of bare rock on their west sides. He ascended one or two other hills of granite situated close to the sea, and up to a height of about 380 feet. On these hills he found abundance of smoothed rock surfaces sloping down to W.N.W. In one case only, the direction was somewhat abnormal, viz., west by north.

About half a mile to the east of Dalbeag farm-house there is a steepish bank facing the sea (which is due west, and only a quarter of a mile distant), surmounted by a cliff, as shown in fig. 33. The bank is about 50 feet high, and is covered by boulders and gravel. On the very top, viz., about 285 feet above the sea, the bare granite rock has been planed down and is occupied by a number of boulders. The only boulders which showed direction of transport indicated a N.W. direction.

At Sheabost, a place between Dalbeag and Barvas, notice was taken of a remarkable assemblage of gravel knolls on both sides of the road, but not forming a continuous kaim. These knolls were approximately elliptic in shape, the longer axis being about 50 or 100 yards, their breadth 10 or 12, and their height from 20 to 30 feet. Most of these gravel knolls have their longer axis running in nearly the same direction, viz., north and south. Large boulders lie on these knolls, and mostly on the west sides.

The boulders were in some places piled above one another. The uppermost showed from their position that they had come from the westward. The height of these knolls above the sea is about 130 feet. The distance from the sea-coast is about half a mile.

Nearer Barvas village there is a lake called Urraghay, on the west side of which there is a remarkable assemblage of large boulders, some of them granite, forming a sort of *trainée* running W.N.W. No rock is visible here. The ridge on which the boulders lie seems to be composed of coarse water-borne gravel. One of the largest boulders measured 12 × 10 × 5 feet. Its longer axis lay W.N.W. The uppermost boulders indicated transport from the N.W.

At Shadir, about 4 or 5 miles to the east of Barvas, there is a lake whose longer axis runs N.N.W.; its west bank has on it a considerable number of boulders, at a height of 240 feet above the sea.

At Galston farm and shooting-lodge there are some rocky cliffs, reaching to a height of 120 feet above the sea, bared as usual on the N.W. slopes, and having a few small boulders on these slopes.

A new school was built last year near Shadir, the stones for which consisted entirely of boulders extracted from under the peat. One of the masons employed on the school stated that many of the boulders consisted of Dalbeag granite, a variety which, on account of being better adapted for building than most of the rocks in the island, is well known to the native masons. One of the gateways to Stornoway castle was built of it. Dalbeag is distant from Shadir about 14 miles, and bears from Shadir west by south.

The scarcity of boulders in the district between Barvas and the Ness, when compared with their numbers almost everywhere else in the Lewis, may probably be accounted for by the absence of any ranges of hills in the north end of the island. If the sea stood 1000 feet or more above its present level, with a current in it from the N.W., and this current loaded with ice carrying boulders, it is to be expected that these ice floes, when obstructed in their progress by submarine rocks, would discharge their stony cargoes on these rocks, whilst in the districts where there were no submarine rocks, the current would flow on unimpeded.

12. In the neighbourhood of Stornoway there is the peninsula of Eye, on which the Convener found some smoothed rocks, and some boulders deserving of notice. Smoothed rocks occur to the west of Phabaill village, their smooth sides facing the west. Boulders of gneiss and of a hornblendic rock lie on the moor to the S.W. of the village. The rock *in situ* here is a species of conglomerate or breccia. The gneiss boulders most probably come from the Barvas hills, as they consist of gneiss. The Convener was told of a hornblendic rock, similar to that of the boulders, being on the N.W. shore of the Eye peninsula, but he had not time to go in search of it.

The Convener was informed by Henry Caunter, Esq., a gentleman of scientific knowledge resident at Stornoway, in the employment of

Sir James Matheson, of a sandstone boulder near the brickwork at Garabost, unlike any rock at present known in the Lewis; and he pointed out to the Convener some building stones brought from Loch Broom on the coast of Wester Ross, which he thought exactly resembled the rock composing the boulder.

As the occurrence of this sandstone boulder at Garabost is of importance, by its bearing on the question of transport, the Convener made a special inspection of it.

The Convener, having been introduced by Mr Caunter to M'Fadzyen, the manager of the brickwork, was taken by the latter to the boulder, and was informed by him that some years ago it had been partially blasted with gunpowder for building purposes. It had originally weighed about 8 or 9 tons, but the lower half still remained, showing its shape and position. The boulder was a coarse brown sandstone, full of quartz pebbles about the size of a small pea.

The boulder was on the side of a hill sloping towards the sea, on the N.W. side of the Eye peninsula, and facing the west. It was buried in a bed of gravelly clay, which had all the appearance of being a marine deposit, and it was within a mile's distance from Garabost brickwork. The height of the boulder above the sea was about 50 feet, and its distance from the sea about a quarter of a mile.

The Convener found on the surface of the same hill, sloping to the west, another sandstone boulder about the size of a man's head, exactly similar in composition.

The hill on the side of which these boulders were lying, rises up gently towards the S.E. to a height of about 160 feet above the sea.

Now it appears, from what Mr Caunter stated, that no sandstone rock, exactly similar to that of these boulders, had been seen in the Lewis; but, on the other hand, the geological formation or class of rocks to which these sandstone boulders belong, does exist in the Lewis. Dr Macculloch, in his geological map of the West Highlands, indicates, by its appropriate colour, this formation as occurring for many miles on the east coast of the island, near Stornoway.

The Convener had pointed out to him by Mr Caunter a long range of high cliffs along the shore, to the north and south of Stornoway, of a sandstone breccia or conglomerate, identical in composition with

a breccia or conglomerate occurring on the mainland, and which Dr Macculloch and Professor Nicol ("London Geological Society's Journal" for 1856, p. 37) concur in representing as "*the bottom beds*" of the great sandstone formation which lines the north-west coast of Scotland, and which constitutes the entire mass of a number of small islands lying off the coast, extending from Cape Wrath to Skye, a distance of about 100 miles. Dr Macculloch mentions having observed a similar conglomerate on the west side of the Lewis. ("Western Islands," vol. i. p. 196.)

These breccia sandstone cliffs extend along the east coast of Lewis for about 15 miles. Referring to them, Mr James Geikie ("London Geological Society's Journal" for 1873, p. 534) says that "red sandstone and conglomerate of Cambrian age cover a portion of the Eye peninsula and the shores of Stornoway harbour at Arnish point. The same deposits are continued north as far as Gres."—Gres is about 15 miles to the north of Arnish.

This sandstone formation is not confined to the coast. It extends some distance inland, though how far has not been ascertained. Mr Caunter showed to the Convener a bed of the breccia in the channel of a stream which runs through his garden on the north side of Stornoway. Mr Geikie, in his paper before referred to, suggests that "red sandstone may occupy the sea-bottom at no great distance from Cellar Head, and hence we are not compelled to suppose that these sandstone fragments have travelled from the mainland" (p. 539). The "sandstone fragments" here alluded to by Mr Geikie, are "red sandstone boulders, lying in the fields, which we found at the Butt" (the northern extremity of Lewis), and also on "the sea-beach at Barabhais" (a place about 20 miles from the Butt, on the west coast). Cellar Head is a point on the east coast of Lewis, 5 or 6 miles south from "the Butt."

Mr Caunter told the Convener that he had seen the sandstone boulders on the shore between the Butt of Lewis and Ness, and that they occur there inland up to a height of 300 feet.

Now, a presumption arises, from the number of these sandstone boulders at and near the Butt, that there must be in that district rock *in situ* of the same nature. The Convener regretted having been prevented searching the coast and fields between the Butt and Barvas, to examine these boulders and see if any sandstone rocks

occurred there on the shore. He, however, saw Mr M'Farquhar, the intelligent ground officer at Barvas, and learnt from him that about a mile or more to the west of the mouth of the Barvas river, where it flows into the sea, there are rocks which seemed to him to have the appearance of sandstone rocks, but that he was not competent to judge of such a matter.

In these circumstances, the presumption is that the sandstone boulder at Garabost came, like all the other boulders in the Lewis, from the westward, and not from the mainland of Ross-shire.

13. The Convener (IX., art. 4) referred to the flatness of the district to the south of Stornoway. Between Stornoway and Barvas and also both towards Dalbeag and the Butt of Lewis, the island presents similar tracts of flatness. The general height above the sea is much the same over both districts, viz., from 200 to 300 feet. The deposits forming these extensive plains consist of great sheets of gravel, sand, and stony clay,—the clay being generally the lowest bed. In these flat districts, there is a remarkable scarcity of boulders when compared with their number to the south, and these few are much below the average size.

A great many sections of these deposits were examined for sea shells;—but the only place where shells were seen by the Convener was at the brickwork of Garabost above referred to. These shells —chiefly the *Cardium edule*—have long been an object of interest, and were examined by the late Dr John Davy of London, as well as by Sir Charles W. Thomson and Dr Carpenter. At one time they were thought to be arctic; but the latest opinion is, that they are of the type now existing in the adjoining sea.

Mr James Geikie gives an account of this Garabost deposit in the memoir read by him before the London Geological Society in April 1878. But his account is founded, as he says, chiefly on information supplied by Mr Caunter, whose letter he quotes. As the Convener made a careful examination of this clay-bed, he gives, with the aid of fig. 34, the following description of it:—*a*, is gneiss rock; *b*, is coarse shingle; *c*, is the bed of clay now worked; and *d*, is sand covering the clay.

The Convener picked up fragments of the shells from the bed *b*, as also several well-rounded boulders of gneiss, about the size of a child's head.

The manager of the brickwork pointed out how the upper part of the clay-bed appeared to have been scooped out in some parts; the hollow thus made being filled with sand and mud. The bottom of the clay-bed was not sufficiently exposed when the Convener visited the place, so as to show the bed of shingle; but there was a heap of coarse gravel near the work, which the manager stated had come from the bottom of the clay-bed. The Convener had also explained to him the vegetable remains said to have been found in the upper part of the clay, to which Mr Geikie alludes, as supposed by Mr Caunter to have been "*common sea tangle;*" but of this the Convener saw no specimen.

Mr Geikie mentions that the clay-bed at Garabost is "in all probability of the same, or approximately the same, age as the similar beds in the north of the island" (*Lond. Geol. Soc. Quarterly Journal*, vol. xxxiv. p. 827).

The Convener made an attempt to reach the north of the island, to see those shelly clay-beds referred to by Mr Geikie; but, from want of time, he failed to get so far north. He therefore may be permitted to refer to Mr Geikie's account of these beds, and to quote one or two passages:

"At Port of Ness the boulder clay contains patches of sand. But the most remarkable feature is the presence of broken *arctic and boreal shells,* which occur in an irregular manner through the mass. The *upper surface of the boulder clay is denuded;* a character better shown in fig. 37, which is taken from the same locality. The stratified beds contain *shells, most of which are in a fragmentary state, but some perfect specimens may be detected.* They belong to arctic and northern species." Another place is mentioned where "the beds consist of an upper series of sand and gravel deposits, more or less separated from an underlying deposit of imperfectly laminated dark blue and grey clay, and silt or mud. *Shells occur in both.*" ("Great Ice Age," 2d edition, p. 170.)

These shelly beds of boulder clay, according to Mr Geikie, extend over a considerable tract in the north of Lewis. He states, p. 183, "*The shelly tills in the sea cliffs near the Butt stretch across the island from shore to shore,* a distance of two miles or thereabout, forming a narrow belt of low ground, which does not rise more

than 90 feet or so above the sea. The deposits extend for somewhat less than a mile along the east coast, but on the *west* side of the island one can trace them for a distance of *three miles*."

In connection with this northern part of the island, it is proper to notice several remarkable lines of kaims or gravel ridges and knolls. The Convener's attention was first called to these by Mr Mackay (Sir James Matheson's commissioner), who pointed them out from the high road between Stornoway and Barvas, as a feature of the district he had seen nowhere else. The Convener observed these ridges on both sides of the road, and a few days afterwards he had an opportunity of walking along one of them to the north of the Barvas hills. The ridges consist of gravel and sand, and reach a height of 30 to 50 feet above the adjoining level ground, from which they are the more easily distinguished by the uniformly green colour of the herbage on them, whereas the flat district they traverse is covered with brown peat and moss. Each of these gravelly ridges is continuous for more than half a mile, and they deviate very little from one direction, which is about W.N.W. (magn.) When on the top of the Barvas hills, the Convener was able to trace the line of one of these kaims, for at least two miles, running in a direction N.W. and S.E. It passes Loch Scarabhat at its south end. In several parts of their course, boulders occur on the ridges and sides of these kaims. At one place, two or three miles north of the Barvas hills, to which the Convener was conducted by Mr M'Iver, an intelligent gamekeeper, well acquainted with the district, he found the kaim expanded into a number of grassy knolls, much resorted to in summer for the good pasturage they afford to cows. These knolls were, in some spots, well covered with boulders : the highest knolls being those where the boulders are most numerous. The boulders were sometimes on the east sides of the knolls, but more frequently on the west sides. At two places, the boulders were heaped and piled on one another. The Convener attempted to elicit from their relative positions, the quarter from which they had come. Most of the boulders showed unmistakably that they had come from the N.W., but some also from W.S.W. One boulder indicated transport from N.N.E.

An old man who was looking after the cows at this shieling noticing the attention paid by us to the boulders, volunteered to

mention that robbers used to live in the recesses among the boulders. The Convener's man-servant crept into one of the recesses pointed out, which was so large as not only to admit him, but conceal him when in it from our view.

Another observation by the Convener in connection with this district may be mentioned. On the north side of the middle Barvas hill there is a deep hollow, like a huge trench, close to and parallel with the northern contour of the hill, suggesting the idea, that when the country was submerged, an oceanic current from the N.W. striking on the hill may have scooped out the drift forming the sea-bottom at this place.

14. The Convener was as much impressed as Mr Geikie appears to have been with the number and direction of lakes in the Lewis. In his "Great Ice Age" (2d ed., p. 168), under the head of "Lakes occupying hollows in the till or other superficial deposits," Mr Geikie states,—"They rest sometimes in the hollows between banks of till, and not unfrequently in cup-shaped depressions of sand and gravel. The most considerable assemblage of these lakes of which I know, is in the Island of Lewis; the low lying tracts of which are literally peppered with lakelets. Not a few of these belong to the drift-dammed series. But hundreds of them appear to rest in hollows of the till, their longer axis pointing by N.W. and S.E." The Convener remembers that when on the road from Stornoway to Garry-na-hine, he stopped the carriage to count the lakes spread out before him. They were 17 in number—though seen from a point only about 300 feet above the sea. To the north and north-east of the Barvas hills the lakes are even more numerous.

The Convener also concurs with Mr Geikie in his remarks (*Lond. Geol. Soc. Journal*, vol. xxix. p. 541) that, "with one exception, all the longest and most considerable lakes range in a direction from S.E. to N.W." "They extend in long lines, often for a mile or two, with an insignificant breadth."

When Mr Geikie proceeds to suggest a cause for the formation of these lakes, and for their persistency in a N.W. and S.E. direction, the Convener is unable to concur. He says—"When the ice that swept across the Lewis finally vanished, it left as marks of its power not only rounded and fluted hill tops, but hollows scooped out in the solid gneiss. The till that accumulated below the ice was also

at the same time found arranged in long parallel banks, running in the exact direction followed by the ice striæ and *roches moutonnees.* The arrangement of the till into long parallel mounds is a feature with which I have long been familiar." "The N.W. and S.E. lakes then rest in true rock basins, and also in hollows between parallel banks formed wholly of till, or partly of rock and till" (page 542).

The Convener walked along the banks of many of the lakes in the northern part of the Lewis. He does not remember having seen much or indeed any rock on those banks. At all events, the banks certainly in most cases consist of gravel and till, forming "*long parallel mounds*," as stated by Mr Geikie. On some of the heights, as at Bein-na-Bhuna, there are domes of smoothed rock. But because they are round and smooth, the Convener does not admit that they thereby prove glacier agency. The main facts mentioned by Mr Geikie the Convener quite admits, viz., that most of the lakes are "occupying hollows in the till and other superficial deposits"—that the axis of these hollows is, generally speaking, N.W. and S.E.—and that this also is the direction of the ruts and flutings on smoothed rocks. Mr Geikie assumes that these lake hollows, and these ruts and flutings, were made by one and the same agent, viz., ice, which came from the S.E. The Convener, on the other hand, ventures to suggest that the ruts and flutings may have been made by an agent which came from the opposite direction, viz., the N.W.; and that this agent may have been an oceanic current loaded with ice, which ploughed through the old sea-bottom, pushing hard stones over submarine rocks, which were thereby smoothed and striated.

There is one general view put forth by Mr Geikie with which the Convener agrees. Mr Geikie, after traversing the whole of the Outer Hebrides, from the Butt of Lewis to Barra Head, has formed an opinion that the phenomena of smoothed and striated rocks and boulders in all these islands can be best explained by one agent, which embraced and spread over the whole, and reached up to at least 1600 feet above the present sea-level. The Convener concurs in that view. In all the Hebrides which the Convener was able to visit he found a remarkable agreement in the direction of boulder transport and of rock striations, and in the disposition of superficial deposits. This agreement does certainly suggest the agency of some general agent embracing all the islands. The only

question is, What was this agent? Was it a sheet of ice from Ross-shire, crossing the deep channels of the Great and Little Minch and flowing from the S.E. with a breadth of 120 miles? Or was it an ice-loaded oceanic current from the N.W. when the sea was, say, 2000 feet above its present level?

As reference has been made to the low-lying level plains occurring in the Lewis, and to the beds of sea-shells in the till, it may not be deemed irrelevant to mention that there are on many parts horizontal terraces, bounded by cliffs which seem to indicate old sea-margins. Along the east coast, from Loch Seaforth to Stornoway, there are cliffs at heights of 11, 40, 81, 180, and 220 feet above the sea. The road from Stornoway to Garry-na-hine, for some miles, passes through a valley exhibiting a sea cliff at a height of from 210 to 220 feet. The valley through which the River Barvas flows to the sea, exhibits distinctly two terraces with cliffs, one 40 feet and the other 170 feet, above the sea.

The theory of an ice-sheet from Ross-shire overspreading all the Outer Hebrides is too large a question to be discussed in this Report. But as having an important bearing on the question, the Convener may advert to the way in which the boulders are distributed in these islands. It has been already remarked that boulders are scanty on the east coasts of those islands, and in particular on the low-lying districts in the north of Lewis. It may be supposed that it is only natural that the boulders should be most abundant on the west coasts, as the highest hills are there. But it does not follow that the boulders, because they rest on these hills, were generated there. For example, the large boulder on the west flank of Dun-Ii in Iona, the numberless boulders on the sea cliffs on the west coasts of Tiree, Coll, Barra, Uist, Harris, and the Lewis, must have come from the westward, and been stranded on the first islands, or submarine rocks or shoals, which impeded the farther progress of the ice which brought them. On that theory, it would not be difficult to explain why the boulders, whilst abundant on the mountains which fringe the west coast of the Hebrides, should be generally absent from the eastern and northern portions of the Lewis, where there are no hills, or any other obstruction to the ice in a sea, if one prevailed, about 1000 feet above the present level.

If glaciers ever existed among the hills of Harris, their effects

must have been confined to their own valleys. Though Mr James Geikie, in his valuable Memoir on the Glaciation of the Hebrides, assumes that there were such glaciers, he not only admits but maintains, that "the ice, with which the mountain valleys of Harris and the south were filled, *had no share whatever in the glaciation of the northern part of the island*, extending from the base of the mountains to the Butt, a distance of not less than 35 or 40 miles. Where, then, did the ice come from which overflowed this by far the largest part of the island? There is only one place whence it could have come,—the *mainland*." Mr Geikie "contends that it was amongst" the "mountains of Wester Ross, fringing the borders of the Minch, that the glaciers which overflowed the Lewis were nourished" ("Lond. Geol. Soc. Journal" for 1873, p. 544). In his second Memoir, read in April 1878, Mr Geikie extends this theory to all the Outer Hebrides, maintaining "that *the whole of the Long Island, from the Butt of Lewis to Barra Head*, has been overflowed from the Minch by ice that moved outwards from the inner islands and the *mainland*" (p. 861.) If this had been the case, one would have expected to find boulders chiefly on the *east* coasts of the Hebrides, and few on the *west* coasts. But the facts are entirely the other way. Not only is it on the hills of the *west* coasts that boulders most abound, and are largest in size; but it is also on the slopes of the hills facing the Atlantic that these boulders are mostly seated. On the hills of the east coast next the Minch, the boulders are few and small, and they are chiefly on the west flanks of these hills, and therefore unlikely to have come across the Minch.

XI.—OBAN AND ITS NEIGHBOURHOOD.

In the immediate neighbourhood of this town, there are some facts of interest.

(1.) There, as among the Hebrides, the smoothed rocks on the hills above Oban face the N.W.

A little above the Craig-Ard Hotel, there is a fissure in the hills from 12 to 20 yards wide, and running due north and south for 200 yards, at an elevation above the sea of about 180 feet. The fissure has apparently been occupied by a trap dyke, which, from the sea or other natural agencies, has decayed and disappeared. The walls of

the fissure are from 12 to 20 feet in height. The east wall of the fissure presents numerous portions of rock well rounded and smooth. The west wall is rough and jagged. These appearances suggest the action of a current which has grated on the east wall and not on the west wall.

As this is a case exactly similar to that referred to as occurring in Barra, and shown by figure 11, it is unnecessary to give another diagram.

(2.) Not far from the foregoing spot there is a coarse-grained conglomerate rock. It is at the junction of three roads. It is the same species of rock which forms what is called the Dogstone on the avenue to Dunolly, at Oban. The included boulders and pebbles are well rounded, and consist of hard gneiss and quartzite.

Fig. 35 represents a portion of this conglomerate rock,—about 20 feet across—viz., between east and west, and 5 feet between north and south. On the side of the rock facing the N.W. the hard pebbles and boulders in the rock have all been ground down to an even surface; whilst on the side facing the S.E. the pebbles and boulders retain their original shapes, and stand up above the clay matrix of the rock.

(3.) About 6½ miles from Oban, at a place called "Lailt," there is a boulder of considerable size called "Clach-a-Currail" or Perched-up Boulder. Its height is 14 feet, and its girth about the middle 29 feet. Its situation is extremely critical, being on the edge of a precipice which goes down at an angle of about 75° for 50 feet. The rock of the boulder is peculiar,—a dark chocolate-coloured porphyry. No rock of that description elsewhere could the Convener hear of.

How the boulder got into the site it now occupies, or from what quarter it came, it would be difficult to say. Judging from the position of the boulder, the presumption is that it came from the S.E., *i.e.*, down the valley leading up to Loch Awe. But it may have come from the N.W., as in that direction there is a valley by which it could have floated to its present position.

About half a mile to the N.E. of this boulder there are fragments of what had been a much larger boulder, which a year ago had been blown up for building purposes. It was a coarse granite, whilst all the rocks in the district are gneiss. Its position suggested trans-

port from the S. or S.W. as the most probable quarter, though the N.W. was not impossible.

Most of the small hills in this neighbourhood are bare on the N.W., and are smoothed on that side only.

(4.) The Convener paid a visit to a glen called Glenlonnan, the mouth of which comes down to Loch Etive near Taynuilt. He had been told of there being several large boulders on a hill called Bein Glas in that glen, about 1700 feet high. This is the glen referred to in the last Report of the Committee, page 12 and section 5. He was guided to these boulders by Mr Clerk, a son of the tenant of the farm of Duntonichan, of which Bein Glas forms part. In ascending the north flank of the hill, it was observed that the smoothed rocks here as elsewhere distinctly sloped down towards the N.W., and that rounded boulders were often on these rocks. The rock of the hill was gneiss, and most of the boulders were also gneiss; but there were also some of granite, a few of mica slate, and a very small one of quartzite. The largest granite boulder passed measured $6\frac{1}{2} \times 4 \times 3$ feet. The longer axis pointed N.N.W. The rocky surface on which it rested dipped due north.

When a height of 1619 feet was reached, which was near the top of the hill, it created some surprise to find that there were smoothed rocks facing the *south*, besides others facing the north.

Several large boulders were found occupying positions on slopes facing the south. One of these was a well-rounded grey granite, at a height of 1573 feet.

At a height of 1637 feet there was a boulder, $8 \times 5 \times 5$ feet, very angular. It was a dark purple claystone, in appearance similar to the boulder, shortly above mentioned, seen at Lailt. It was resting on a shelf of gravel, but the general slope of the hill was exceedingly steep, viz., forming an angle of about 35°, sloping down S.E.

Judging by the position of these boulders, and the steepness of the hillside facing the S.E. or the S.S.E. which they occupied, the presumption is, that they had come from that direction. The rocks of the hill at this spot are also smoothed in that direction. Towards the south there are high mountains in the distance.

Another grey granite boulder, $3 \times 3 \times 2$ feet, was found at a height of 1645 feet on a rocky slope, less steep, but still facing the south.

At a height of 1683 feet, about 11 feet below the summit, smoothed rocks were still found sloping gently towards the south.

On descending the hill towards the north, by a more westerly path than that followed in ascending, it was observed that, at a height of 1554 feet, the smoothed rocks faced the north.

Some of the boulders met with on the descent were of the same dark purple porphyry seen at Lailt.

(5.) On reaching Loch Etive, the Convener visited the Airde point, a projecting cape or headland on the west side of the loch. At this point there were many well-smoothed rocks up to a height of 276 feet above the sea, and facing up the glen towards Loch Awe. There can be no doubt that these rocks had been smoothed by glacier action. On this Airde point there were numerous boulders, chiefly of grey granite. They may have been pushed down the glen by a glacier;—indeed it seemed the most probable supposition. But they might have been floated up from the N.W. None of the boulders seen were in such a position as to indicate with any certainty the quarter from which they had come.

It may be added that the rocks on the south shore of Loch Etive, as far down as Connel ferry, and even lower, show smoothings all facing up toward the head of the loch, suggesting glacier action from the upper part of the valley.

(6.) In the Fourth Report by the Committee (p. 11), reference was made to boulders observed in the Island of *Kerrera*, at the north end. This year the Convener had an opportunity of examining the boulders in the middle of the island, where it is traversed by the high road leading from Ballimore farm to the ferry for Mull on the west side of the island, called "Bal-na-Bok."

On his way across the Island, he had pointed out to him by Mr M'Dougal, tenant of Ballimore farm, three or four well-rounded boulders of a coarse granite, having a red tinge, imparted from the felspar crystals. They were from 2 to 3 feet in diameter. Mr M'Dougal stated that there was no granite rock in Kerrera which he knew of; and that the nearest place where he had heard that granite of that kind was worked was at Morven, about 12 miles across the sea to the north. He was sure it was not the same as any he had

seen in Mull. He referred the Convener to Mr John M'Dougal, builder, Oban, as one who had a practical knowledge of granite rocks.

On the hill sides facing the north and west, the Convener observed here and there several boulders. They were all mostly of the same coarse-grained granite. There was one of a purple claystone porphyry. When he reached "Bal-na-Bok," he passed several boulders of coarse-grained granite, and one block of mica schist, which had been hollowed out for some domestic use. He learnt from the old ferryman (M'Kinnon) and his daughter, that there were boulders of granite about 4 feet high at or near the tops of the hills to the south of the ferry. Rainy weather prevented access to them.

On returning to Oban, the Convener called on John M'Dougal, the builder, and showed to him specimens of the granite boulders which he had found in Kerrera. On asking him if he knew where there were any *rocks* in the hills of a similar description, he said that he knew of two places,—one to the south of Ben Cruachan, the other in Morven,—and that he thought the Morven rock more nearly resembled the specimens shown.

He was not acquainted with any granite exactly similar existing in the island of Mull. He knew very well the red granite of the Ross of Mull; and he added that, at a place which he called the "North Bay of Mull," there was a grey-coloured granite, much lighter in colour than that in Loch Etive.

In these circumstances, it is still matter of doubt from what quarter these red granite boulders in Kerrera were transported.

(7.) The Convener next day paid a short visit to Easdale, and was conducted by Mr John Clerk, blacksmith, Kilbride, to several places in the neighbourhood, for an inspection of boulders which had been reported by him in one of the circulars to this Committee. The district visited was that traversed by the high road to Clachan Bridge, situated about 3 miles to the N.N.E. of Easdale. The rocks of this district are all a blue clay slate, extensively quarried for roofing. Most of the boulders examined were of grey granite, but their position did not indicate clearly the quarter from which they came. They probably came from the north or west, as there was less in these directions to obstruct them in their transport than in any other direction.

To the south of Easdale, there is an extensive terrace along the coast, about 18 or 20 feet above high-water mark, and some hundreds of yards wide, bounded by a range of high rocky cliffs, with caves which evidently had been formerly reached and undermined by sea waves. On this terrace lay a cluster of boulders, several of them of grey-coloured granite, which most probably had been lodged where they now lie by ice floating from the north, and arrested in its further progress south by these rocky cliffs. Several boulders were noticed by the Convener at and near the tops of these cliffs, which he regretted not having had time to inspect.

Mr Clerk informed him that on the hill immediately to the east of Easdale, about 1200 feet in height, there were near the top several large boulders, which he hoped would be examined at some future period.

Whilst it seemed probable that these Easdale grey granite boulders came from the north, there was one large claystone boulder, of a purple colour, which, from its position, seemed to the Convener to have come from the south. Its size was 12 × 7 × 6 feet. It lay on the shore near Clachan Bridge. On asking Mr Clerk if he knew of any rock *in situ* similar to that of the boulder, he pointed to a hill about a mile distant, situated to the south.

The Convener has referred to several boulders of a purple-coloured claystone, very similar to this one, as having been seen by him at "Lailt" (3) above, and "Duntonichan" (4) above, which also suggested transport from the hills to the south.

(8.) The Convener on 1st July ascended Ben Cruachan from Inverawe, up as far as 2725 feet, and made the following observations:—

Until a level above the sea was reached of about 1330 feet, few boulders were met with. At and above that height the boulders were numerous, and many of them of large size. They were most numerous on the N.W. shoulder of the hill. In that direction there was the least obstruction to transport. Due N., N.E., E., S.E., S.W., due W., there were hills of formidable height which would obstruct. Towards the W.N.W. and N.W. there were only the hills in Mull and Ardnamurchan, distant from 30 to 40 miles.

The possibility of transport by a glacier down from Loch Awe or

Dalmally, was not overlooked. But if any glacier had filled the valley to the height of 1330 feet, bringing down boulders, these boulders would have much more probably been lodged on the hill to the north of Cruachan, called Daranish, opposite to Bonawe, where there is now a great quarry of granite. But on that hill, at least on the side opposite to and looking up towards Loch Awe, only a few boulders were discernible.

On the other hand, if boulders were brought by a N.W. current, the part of Cruachan which would be first and chiefly struck would be its N.W. shoulder, where the boulders now lie in great heaps, whilst that part of Daranish hill, which faces about south by west, would be sheltered from the current.

The following boulders of considerable size indicated by their position that they probably had come from the N.W.:—

One at a height of 1890 feet, resting on gravel, $15 \times 9 \times 5$ feet.

Another at a height of 1943 feet. It was 13 feet long $\times$ 7 feet high. Its longer axis bore N.W. by W. At its west end, its width was 2 feet, at its east end 5 feet. It also lay on gravel and small boulders.

At a height of 2194 feet, the rocks of the hill—a coarse reddish granite—presented extensive smoothings facing W. by N.

At a height of 2386 feet, there was a boulder $7 \times 6 \times 5$ feet, evidently blocked on its E.S.E. side by the rock of the hill.

At a height of 2428 feet, a grey granite boulder was found near a summit level, where the rock—a red or yellow felspar—showed smoothings from the N.W.

The hill to which these observations apply was not one of the central peaks of Cruachan, but situated to the N.W. This hill is known in Gaelic by a name which in English means "hill of the horse heel." Its top was not reached by about 100 feet. Boulders were, however, descried on it reaching to the very top.

Descent from this hill was made on the side next to Cruachan, *i.e.*, on its S.E. and S. side. No smoothed rocks were observed on these sides, and but few boulders.

If a glacier descended the valley from Loch Awe, grating on Cruachan, it is natural to suppose that the rocks on these flanks of Cruachan would have shown some smoothings. There is a

vertical cliff of rock, about 60 feet above the River Awe, on Cruachan, which did suggest glacier friction; and at a height of 334 feet above the sea, above Inverawe, the Convener found rocks which seemed to have been smoothed from the W.S.W. But above that height the rocks presented smoothings successively from N.W. by N., from N.N.W. and W.N.W.,—the W.N.W. being in the highest parts of the hill, apparently the most persistent direction.

(9.) The Convener afterwards proceeded to the head of Loch Etive in a steamboat, and then travelled by coach nine or ten miles to the head of Glen Etive, to a height of about 600 feet above the sea. The whole of this valley has at one time been filled with gravel and boulders of grey granite. A great part of this mass of drift had apparently been scoured out by the action of the numerous streams which descend from the high steep mountains on each side of the glen. Terraces were occasionally visible on the south side of the glen, up to a height of about 500 feet above the present channel of the river, consisting of clay, gravel, and sand, which may have been the bottom of an estuary in former times.

XII.—LOCH CRERAN.

The Convener paid a visit to Loch Creran, having last year seen that there were there more objects of interest than he had then been able to overtake.

At the mouth of Loch Creran, where it joins the Linnhe Loch, the rocks are all smoothed when they face the W.N.W. at about 70 feet above the sea, and also at Craigan Ferry. But about a mile higher up the loch, the smoothed rocks face W.S.W., at a height of about 80 feet above the sea.

Near the sea-level, the smoothing of the rocks seemed attributable to the action of some force moving down the valley, whilst rocks at a higher level, say 100 feet and more above the sea, grinding from the N.W.—*i.e.*, *up* the valley—seemed undoubted.

On going up the glen towards Carroban hill, notice was taken of a *trainée* of boulders which appeared to go over a summit level to the east of that hill. The boulders are all of a dark-coloured fine-grained granite, and are apparently the same as the Fasnacloich and Appin boulders referred to in last year's Report. Mr Hall, the intelligent tenant of Fasnacloich, who from boyhood has lived in

the district, mentioned that the *traînée* of boulders could be followed for some distance over the hill, towards Glen Etive and Glencoe.

A very large boulder exists in a *cul de sac* formed by lofty hills near Carphin at the head of the valley. It goes by the name of the Ardshiel boulder, in consequence of having been made use of by the proprietor of Ardshiel for concealment in the time of the Rebellion. This boulder is $40 \times 27 \times 15$ feet = about 1000 tons.

A fissure exists through the middle of it, which is large enough to allow of a man getting into it from the top, where, however, the fissure is not discoverable at any distance in consequence of beech-wood growing on it. This boulder is, in composition of rock, the same as all the rest of the boulders in the glen, and it has undoubtedly been floated like the rest, up the glen. It is blocked on its west side by a large mass of rock, which stopped its further progress up the glen. Its height above the sea is 506 feet. On account of its weight, the ice which rafted it was probably so deep in the water that it could not get over the summit level by which smaller boulders passed to the east of Carroban hill.

The boulders in Glen Creran are mostly on coarse gravel. At one place above Salar House, there is a cluster of boulders on a rocky knoll.

The summit level on the east side of Carroban hill is about 800 feet above the sea. If the sea, when these boulders were being transported, stood, say 2000 feet higher than at present, any current from the N.W. would flow through and over that Carroban pass. On that supposition, it would not be difficult to account for the *traînée* of boulders in Glen Creran and for the presence of the gigantic boulder in the *cul de sac* at Carphin.

If Robert Hall's statement that the black granite boulders are traceable up the Carroban valley, and over the summit level which separates Glen Creran from Glen Etive, some of these boulders should be found in the upper parts of Glen Etive. As the Convener passed up that valley on the coach which travels between Loch Etive and Glencoe, he observed several boulders on the moors, near the road, exceedingly like the Loch Creran boulders; but he had no opportunity of particularly examining them.

With the view of so far testing the statement by Hall, the Con-

vener at a subsequent date walked across the moors from Ballachulish Hotel to Carroban hill, to see if there were any black granite boulders in that quarter. He fell in with several at a height of about 800 feet above the sea, and he saw that boulders were thickly spread up the valley to the summit level, but unfortunately, he was prevented reaching them for examination on account of distance.

It is worthy of remark, however, that in this side valley, running up from Glen Creran, grey granite boulders are also numerous, whilst in Glen Creran itself there are none. Now, at this place the rocks *in situ* are slate rocks. The nearest mountain of grey granite is situated to the N.W., about four miles distant. A N.W. current would bring fragments of rock to the place where the Convener found them, but not to Glen Creran, at least to its lower parts.

XIII.—GLENCOE.

This valley is quite as remarkable for objects of geological interest, as for picturesque scenery and for stirring historical deeds.

It contains many phenomena of extreme importance, connected with the transport of boulders and the grinding down of rocks.

The Convener began his examination of the glen, at "Alt na Fay," a place about 17 miles distant from Ballachulish Hotel, and about 3 miles distant from King's House.

Having introduced himself to John Matheson, a young shepherd residing at "Alt na Fay," the Convener obtained his services as a guide for some distance down the glen.

The first place visited was a gravel knoll, near Matheson's house, having on it a cluster of boulders, the largest and uppermost being well seen from the coach road. Its size is 8 × 4 × 4 ft., and it consists of a hard clay slate similar to that of the neighbouring hills in the north. Underneath this boulder, there was one of smaller size, consisting of a red felspar, of which, as Matheson informed the Convener, there was also rock in the hills. But there were no hills within a quarter of a mile of this gravel knoll, and no cliffs from which the boulders on it could have fallen. The top of the knoll was about 30 feet above its base, and was of a somewhat conical shape. An examination of its *side*, showed numerous boulders, half buried in the gravel composing it. On the *west* side, there were from

twenty to thirty boulders, on the *east* side only one or two. The greatest number consisted of grey granite—of which rock, however, as Matheson assured the Convener, there was none in Glencoe; the nearest being, as he said, on the shore at Ballachulish Hotel, at the mouth of the Glen, and farther westward towards Duror.

Matheson then conducted the Convener *down* the valley on the north side to some larger boulders. Several were pointed out from 10 to 12 feet long, at heights of 1200 to 1400 feet above the sea. These also were of grey granite, their longer axis being about east and west, or parallel with the direction of the glen, at this place, and about 400 feet above the bottom of the glen.

About 50 feet above these grey granite boulders, smoothed rocks were observed. There were no striæ; but the smoothing seemed due to a frictional agent which had come *down* the glen.

On the same (north) side of the valley, the Convener had pointed out to him by Matheson, at one or two places, about 1183 feet above the sea, a mass of conglomerate rock *in situ*, similar, as he said, to the "Dog-stone" at Oban. The Convener observed that two fragments had been detached from the rock, and been formed into boulders. One was on the slope of the hill, about 50 yards *west* of the parent rock, and 30 feet below it in level; the other of these boulders, and larger in size, was resting on the schist rock of the hill, about 200 yards *west* of the parent rock, and about 45 feet below it in level. These observations indicated that some agent had here been *moving down the glen*, and had both broken off and transported portions of the conglomerate rock.

On the other hand, there is a cliff of this conglomerate rock which holds in a cleft of it a grey granite boulder, and in a position which shows that *it* had *come up the glen* from the west. Fig. 36 A represents this boulder leaning on the hill rock, the view being taken from the south, about 500 yards distant. Fig. 36 B represents the same boulder, viewed from the north, at a distance of about 10 yards.

Matheson next informed the Convener that if the latter wished to see the biggest boulder in Glencoe, he would have to cross to the opposite side, at a place about a mile further down, and at a considerable height above the river channel.

The Convener went to the place and found the boulder in

question. A path from the cottage occupied by Buchanan (a shepherd) led to it. The boulder being of very irregular shape, its exact dimensions were not ascertained. Its girth at the level of the ground was ascertained, by walking round it, to be 22 yards. Its height seemed to be about 15 feet. The rock composing it was a coarse conglomerate. It was resting on a flat or terrace of gravel. Its height above the channel of the river was about 450 feet, and above the sea 1215 feet.

The position of the boulder seems to be indicated on the Ordnance map by the words "Meannar Clach."

The Convener was unable to form a distinct opinion on the question, whether this boulder had come down the glen, or had come up the glen. Its height above the sea was nearly the same as the conglomerate cliff higher up the glen, before spoken of. But, if a fragment from that cliff, it must have crossed the valley. The following considerations favoured the idea that it had been floated *up* the valley. It was resting on the shoulder of a hill facing the N.W.; and on the same shoulder there were multitudes of smaller boulders of conglomerate rock, apparently due to the same mode of transport. A plan of the position is shown on fig. 37, where B represents the big boulder. Some boulders, apparently of a similar character, were visible at A, though they were not visited. If a N.W. current, bearing boulders, came up the glen, it might lodge the boulders at A and B. The Convener believes that conglomerate rock occurs near the foot of Glencoe, as, when there, he saw fragments which appeared to have fallen from a cliff. If this be the case, the theory which ascribes transport of these boulders *up* the glen would be strengthened.

It was observed, that the above "big boulder" rests on a terrace of gravel. It is rather a bed of stony clay, as such seemed to be the character of sections cut through by streams for about 200 feet above the boulder; and this stony clay contained numbers of pebbles and small boulders. It was plainly a water deposit. Above this stony clay, there appeared to be extensive beds of sand; and on several of the hills, near the foot of Glencoe, even up to the height of 2000 feet, sand in large quantities was observed; but it was only through a telescope that the observation was made.

About half a mile below Buchanan's cottage, at the ninth milestone from Ballachulish, the Convener observed a rock well smoothed

and striated; it was at the side of the highroad. The surface of the rock sloped due south at an angle of about 15°. The striæ had a direction N. 55° W.; whilst the axis of the valley here was N. 65° W. There was nothing to indicate whether the striating agent had moved up or moved down the glen.

One of the most interesting spots in Glencoe is where the valley is narrowest, *i.e.*, where the hills on each side approach so near, that their respective rocky cliffs front each other at a distance of only about 300 yards. This narrow defile occurs about a mile to the west of Buchanan's cottage. The river here has cut through the slaty schist rocks to a depth of about 60 feet.

Fig. 38 will give some idea of this defile. There is a large plateau of rocks, consisting of slaty schist, which has been evidently ground down by a heavy body or bodies passing and pressing over it from the *east*, *i.e.*, down the valley. There are elongated shallow hollows also on these rocks parallel with the axis of the valley, which hollows are near the middle, as if the pressure there had been much greater (viz., at A and B) than higher up at C. The smoothing and the hollowing seem to have commenced on the *east* side, as the edges of the strata are mostly smoothed on the edges facing the east. On the west side they are somewhat rough and jagged.

The figure represents a boulder lying on the smoothed rocks on the north side. It had not fallen from the cliffs. If it had, it would assuredly not have stuck in its present precarious position. It is a true erratic, and must have been brought up the glen at a period subsequent to the smoothing of the rocks. The surface of the rock on which it lies, slopes down towards the west.

About a quarter of a mile lower down the glen another smoothed rock occurs, which in like manner shows frictional agency over and upon it *from the eastward*. The rough parts of the rock face the west, and there form a cliff about 50 feet high, which has evidently stopped a number of erratics in their progress up the glen, as they lie in great numbers at the foot of the crag, some resting on others. Fig. 39 represent these boulders, showing how they have been obstructed, and how the uppermost boulder of the two must have come from the west to obtain its position above the other. The rocks in the cliff are a reddish felspar. The boulders are a fine-grained gneiss.

The Ordnance Surveyors having reported to the Boulder Committee a very large boulder seen by them at the foot of Glencoe, and having had the goodness to indicate its exact position on their map, the Convener made an attempt to find and examine it. On the map the boulder is indicated by the name of "Craig Bhatan," which it is believed means "*rock with trees.*" The Convener saw the boulder, at the distance of about a quarter of a mile, with bushes growing on it; but he was prevented reaching it, in consequence of being unable to ford a river between him and it. The size of the boulder was stated by the Ordnance Surveyors to be 90 feet in circumference, and about 10 feet high, and it appeared to the Convener to be of that size. It lies in a meadow adjoining the River Coe, about half a mile to the S.W. of the Glencoe Hotel. The meadow is about 200 feet above the sea, and is closely surrounded by mountains exceeding in height 2000 feet on all sides except two. One of these sides, to the east, is the valley of Glencoe. The other side, to the north, is the valley leading to the sea at Loch Leven, distant about 13 miles.

As the boulder seemed to be resting on an extensive mass of gravel, it seemed to the Convener very probable that it had come from the north, *i.e.*, up the Loch Leven Valley.

On the right bank of the River Coe, nearly opposite the large boulder just referred to, there is a rocky knoll standing from 20 to 30 feet above the adjoining district. This knoll has had lodged on its north side, a number of boulders, whose relative positions indicate transport from the north, *i.e.*, up the glen. One of these is a dark micaceous rock, glistering with abundance of mica. A few hundred yards to the north of the knoll, there is a rocky conical hill, reaching to a height of about 90 feet above the adjoining district. It is on the map called "Tom a Grianain." It consists of vertical strata of mica schist,—the only place where, in the course of this day's perambulation, that kind of rock was seen. There can be little doubt, therefore, that the mica schist boulder just mentioned had come from "Tom a Grianain," *i.e.*, from the north, and been torn from the hill by floating ice.

The facts ascertained in Glencoe seem to indicate two separate agencies. In the first place, there was a glacier, which planed down the rocks, so as to produce the extensive smoothings and groovings

seen at the narrow defile and elsewhere. In the second place, and subsequent to that epoch, the whole of the mountains in this district underwent submergence beneath the sea, in consequence of which not only was the Glencoe valley filled and choked with gravel, clay, and sand; but the highest hills adjoining were under water, and subjected to a great sea-current, loaded with ice, which flowed from the N.W. This glacial current brought from hills in the west, fragments of rocks from these hills, and dropped them in the valley at various points.

XIV.—GAIRLOCH.

In this district, the hills adjoining the coast present on their west slopes, even to their tops, numerous examples of large boulders.

1. Fig. 40 indicates the hills immediately above the Hotel, with coloured dots to represent the boulders on them.

One of these, the Convener found to be in the position and of the dimensions shown in fig. 41.

Its height above the sea is 675 feet; but the important feature is that it is on the verge of a precipice, which goes sheer down vertically about 100 feet. The boulder is a coarse-grained reddish brown sandstone, entirely different from the rocks of the hills, which consist of a slaty schist—being a variety of gneiss. The longer axis of the boulder lies N.W. by W.

There are several other boulders visible along or near to the verge of the cliffs, most of them consisting of the same sort of sandstone, and some consisting of a reddish granite—all evidently erratics.

On the lower slopes of these hills, facing the west, there are hundreds of similar boulders. They are mostly rounded at the ends, and in that respect are quite distinguishable from the rock fragments lying also on the hill slopes, which have fallen from the cliffs above.

2. To the N.E. of Gairloch Hotel there are other perched boulders. Fig. 42 shows one of them resting on a small ledge of gneiss rock, whose general slope is due west at an angle of about 50°. It rests on the rock only at its east end; the west half for about 5 feet does not touch the rock of the hill at all. Its height above the sea is 657 feet.

This boulder could have been deposited on its narrow site only by floating to it from the westward.

3. Fig. 43 represents a boulder on another hill near Gairloch, 747 feet above the sea, on the edge of a high cliff facing the west, and partially resting on two small boulders at its east end. It is a coarse-grained granite, whilst the rock of the hill is a schistose gneiss. It projects $2\frac{1}{2}$ feet beyond the edge, and it in like manner could not possibly have obtained its position except by being brought from the west.

4. Fig. 44 represents a hill about one mile N.E. from Gairloch Hotel, at the top of which (585 feet above the sea) two boulders attract notice. The largest is a block of close-grained Silurian rock, blue in colour and very hard. The smaller is a small block of reddish-brown sandstone, with minute pebbles in it of quartz and felspar. The rock of the hill here is a bluish clay slate, the strata of which are almost vertical.

Fig. 45 is a representation of the largest of these boulders taken from its N.E. side at a distance of about 10 yards. The Convener, on a minute examination, found that the boulder was resting on the rock of the hill, at three points, and that at the lower end it projected 2 feet beyond one of the points of attachment. The boulder sloped down towards the N.W. at an angle of 15°. The points of attachment to the rock seemed so slight that the Convener thought he would have little difficulty with a crowbar in precipitating the boulder down the precipice.

The red sandstone boulder is about 10 yards distant from the large boulder, and lay on a rocky surface facing the W.N.W.

5. Near the foot of the hill just referred to, there is a rocky knoll on the top of which a number of true erratics are clustered. The uppermost is $6 \times 5 \times 3$ feet in size, and lies in such a position over the others as to show that it had most probably come from the N.W. This cluster is shown on fig. 46.

6. There was only one place where striæ on a smoothed rock surface were observed. It was about half a mile to the N.E. of Gairloch Hotel, its height above the sea 340 feet. Fig. 47 represents this rock surface. It slopes about due west at an angle of 30°, till it comes to a nearly vertical cliff. The boulder is 10 feet high, 6 feet wide, and about 4 feet thick. It is within 2 or 3 feet of the edge of the precipice, which is about 50 to 60 feet high. On one side of the boulder, several striæ are visible, running E. by N. They had apparently com-

menced at or near the edge of the precipice, viz., at their west ends, as they are deeper and wider at that end than at the east end.

Betweeu Gairloch and Loch Fionn, a distance of about 10 miles, all the hills have abundance of boulders on their sides up to their tops, and generally these are most numerous on the west sides; but at one place, 600 feet above the sea, two boulders were observed on a rock surface sloping towards the W.S.W., and which apparently had come upon the hill from that quarter.

At first the Convener was surprised to find that the smooth rock surfaces, and some of the boulders in the district between Gairloch and Loch Fionn indicated agency not from the N.W. but from the W.S.W. He ultimately saw an explanation of this deviation from the normal direction, by the existence of a high range of hills due east of Gairloch, which might have deflected a N.W. current, and caused it to flow E.N.E. instead of S.E.

It has been mentioned that most of the boulders on the hills near Gairloch are composed of a reddish brown sandstone rock with small pebbles in it, and that this rock is entirely different from the rocks of these hills.

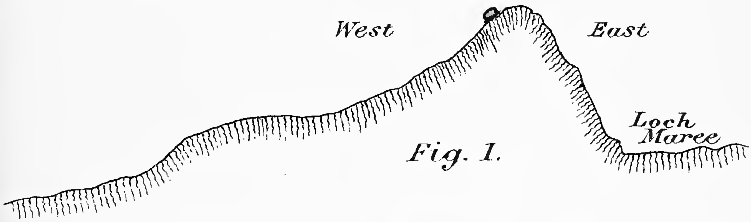
This reddish-brown sandstone rock exists largely *in situ* along the the coast to the N.W. of Gairloch. Professor Geikie, in his recent Geological Map of Scotland, states this to be the case. It is also spoken to, as existing in that quarter, by Professor Nicol and by Robert Chambers. There can be no doubt, therefore, that these Gairloch sandstone boulders, as seen by the Convener at levels exceeding 700 feet above the sea, have come from that district—as indeed the boulders themselves indicate alike by their situation and their altitudes on the hills.

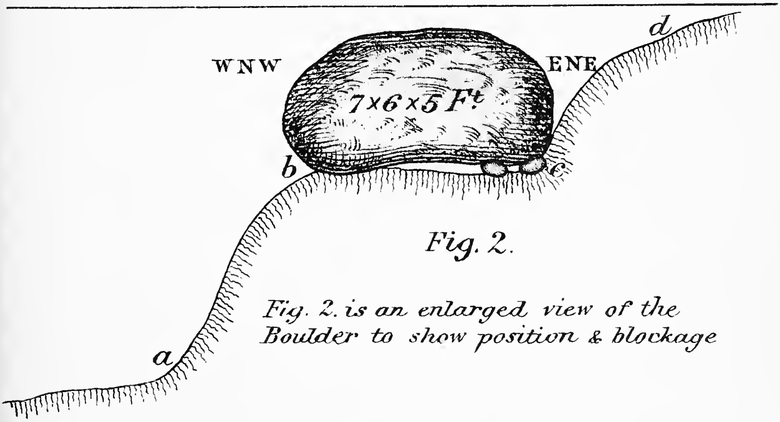
XV.—LOCH MAREE.

The road from Gairloch to Loch Maree passes through a valley running for a mile or more in a direction pretty uniformly W.N.W. and E.S.E. At several places on the roadside smoothed rocks were observed with striæ running in that direction. There was nothing to show whether the rock had been smoothed and striated by a glacier or by sea ice.

On the hills to the west of Loch Maree Hotel, reaching to a height of about 1000 feet above the sea, multitudes of red sandstone

boulders occur, more particularly on the N.W. sides of the hills and on their tops. On one hill (950 feet above the sea), presenting on its top a nearly level surface of about 80 yards diameter, the Convener counted twenty boulders, each exceeding 2 or 3 feet in diameter. Most of these were a coarse pebbly sandstone, the same in its general character as the Gairloch boulders; whilst there were amongst them, just as on the Gairloch hills, a few of a reddish-coloured granite. These boulders, when on or near hill tops, were





a b c d is a section of the part of the hill on which the boulder rests; *a b* is a clift about 30 feet, nearly vertical; *b c* is a shelf on which the boulder rests; *c d* is a steep ledge of rock against which the boulder abuts at its east end. It there rests partly on rock, partly on small boulders.

lying on the bare *well-rounded gneiss rock*, and when on the sides of hills, were generally *on* or *in* beds of coarse gravel. One of the boulders, 4 × 3 × 3 feet, lying near a hill top on the N.W. side, had its longer axis pointing also N.W. On another gneiss hill (also west of the Hotel), and at about 310 feet above the sea, a sandstone boulder was found perched near the top, at its west side, as shown in the above section (figs. 1 and 2).

Professor Nicol, in his paper on the "Rocks of the North-West of Scotland,"* with reference to the hills about Loch Maree and Gairloch, adverts to their being "still strewed with innumerable fragments of red sandstone, perched, like sentinels, in the most exposed and perilous positions, on the very edge of some lofty cliff, or on the polished summit of the domes of gneiss." In a footnote he remarks it as "a curious fact that, on these gneiss hills, by far the majority, probably nine-tenths or more, of these 'perched blocks,' are red sandstone."

The fact would be "curious," if these sandstone boulders had been, as Professor Nicol supposed, "floated on icebergs from the mountains from the east" (page 39), because, to the east of Gairloch and Loch Maree there are no mountains of red sandstone. Professor Nicol, in this paper particularly adverts to "the red sandstone as forming a narrow band along the western shore, never reaching to the watershed of the country." Again (page 37), he repeats, that "the red sandstone on the west forms a narrow band along the shore, and never extended far into the interior."

That being the case, it would indeed be "curious" if the red sandstone boulders which cover the hills about Gairloch and Loch Maree had all been "floated on icebergs from the east." But assume that they had been floated from the N.W., and an explanation is at once obtained.

A curious belt of sandstone rocks occurs to the south of Loch Maree Hotel. Through and across this belt the high road passes for about two miles, so that an excellent view is obtained of the remarkable dislocations and denudations of these rocks which have occurred. These rocks differ in many respects from the rock of the sandstone boulders to which reference has just been made.

Professor Nicol explains that the sandstone of the west coast is "a coarse *grit*, graduating into a *fine conglomerate*, with fragments rarely an inch or more in diameter" (page 19). That is the character of the rock, forming the boulders; but the sandstone rocks which occur near the south end of Loch Maree are correctly described by Professor Nicol as "a very remarkable *breccia* of quartz and gneiss in sharp, angular fragments," the largest of which fragments noticed by him he measured, and found it to be "16

* Proceedings of the London Geological Society, for 1856, p. 29.

inches long by 9 broad and 7 thick, but the generality are much smaller" (page 28).

Referring to this peculiar rock, Professor Nicol observes:—"The red sandstone in this district has undergone enormous denudation. On the shore of Loch Maree it is often broken up into huge masses or divided by gaps and fissures, some of them 20 to 30 feet deep. The surface of the beds is strewed with immense angular and ruin-like blocks, some of them poised on a single corner on the very edge of a cliff. All this indicates extensive destruction of the strata. Detached fragments of the breccia are found in hollows of the gneiss hills, far from the main masses evidently left there in the general denudation."

Now in what direction were these "fragments carried," and from what quarter did this "general denudation"—this "enormous denudation"—come?

The following facts leave no doubt on the subject:—The smoothed surfaces of these breccia rocks face N.W.; the rough sides are towards the S.E. Fragments lie on the surface to the south, beyond the line which separates these rocks as a formation from the quartzite rocks of Ben Eay. Within the limits of the formation, huge masses, weighing hundreds of tons, are lying at the north base of cliffs—not having fallen from these cliffs, but apparently brought there from the north, and left there, in consequence of baving been obstructed and arrested by the cliffs in their further progress to the south.

These facts are in entire consistency with the theory of a strong ice-laden sea current which flowed from the N.W. The valley now occupied by Loch Maree, with mountains on each side exceeding 1000 feet in height, happens to run N.W. and S.E., so that when this district was submerged, a glacial current flowing from a northerly point would produce all the effects on these breccia rocks which have been described.

Before passing from the district of Gairloch and Loch Maree, the Convener thinks it only due to his friend the late Robert Chambers to advert to the observations which he made in districts adjoining to the north. He makes this reference, as the facts observed by Chambers have a close relation to those which the Convener has just been describing.

Dr Chambers' paper was read before this Society in December 1852, and was published in the "Edin. New Phil. Journal" for 1853. The author's chief object was to point out that there were, in his opinion, two sets of phenomena in regard to boulders and smoothed rocks. One set he considered to be the effects of local glaciers, the other he ascribed to a general glaciation of the entire country.

The Convener does not mean to discuss this theory. He wishes only to notice the facts which Dr Chambers brought forward in support of it.

Dr Chambers states—

1. That on Cuineag and Canish (quartz hills in Assynt, situated about thirty miles to the north of Gairloch), he found, "up to a height of 1700 and 1800 feet, striæ running from about N. 60° W., with certain exceptions. One of these exceptions was at the base of Cuineag, where the streaks are from the direct north, apparently by reason of the turn which the agent had there received from the base of the adjoining hill. Another exception was at the hollow dividing the mass of the hill from its loftiest top, where another system of streakings had come in from the direct west."

2. "On a summit south from Ben More, fully 1500 feet high, and four or five miles to the S.E. of Cuineag, there are streakings on the quartz, observing the normal direction of this general movement, viz., from N. 60° W."

3. On the gneissic platform between Coul More and Suilvean (south part of Assynt), Dr Chambers says he "found polished surfaces striated from N.W. to W. To the west and north of the latter mountain are markings in all respects similar. These are situations where no local glaciers could exist."

4. "Streaking, precisely the same as that of Cuineag and Canish, exists at an elevation of at least 2000 feet on the similar quartz mountain named Ben Eay, south of Loch Maree, and forty miles from Assynt—this striation being from N.W. or thereabouts, and totally irrespective of the form of the hill."

5. "Passing northward to Rhiconich, we find near that place striæ coming in from the coast, viz., from the N.W., and passing across a high moor, with no regard whatever to the inequalities of the ground."

6. "A little further north, at Laxford, a fine surface is marked with striation from the N.W., being across the valley in which it occurs. At an opening in the bold gneissic coast which looks out upon the Pentland Firth, there is strong marking in a direction from N.N.W."

7. "The high desolate tract called Moin, between Loch Eribol and Tongue bay, where there is nothing that could restrain or guide the movement of the ice, exhibits striation from N. 28° W.

8. "Striæ, N. 25° W., occur four miles to the east of Tongue bay."

Thus at all these localities north of Gairloch visited by Chambers, the rock striations were such as to satisfy him that some vast agent from the N.W.—*i.e.*, from the Atlantic Ocean—had struck upon the country, and left its marks on hills up to a height of at least 2000 feet above the present sea-level.

XVI.—STRATHGLASS AND GLEN URQUHART.

The Convener, in September last, proceeded up Strathglass, with the view of ascending the mountain called "Mam Saul," about 3880 feet high, in quest of what was supposed by the Ordnance Surveyors to be an old sea terrace. Weather both stormy and hazy defeated this attempt; but whilst in the district, the Convener, accompanied by Mr Jolly of Inverness, made some observations perhaps not undeserving of being recorded.

On the hill above Affric Hotel, on the east side of the River Cannich, a rock was met with, planed and striated, at a height of 450 feet above the bridge across that river, and about 720 feet above the sea. The striæ were running north by west, a direction coinciding with that of Cannich valley.

At a height of 970 feet above the sea a granite boulder was lying on the upturned edges of the gneiss rock of the hill, lying in such a position as to indicate that it had probably come from a west by north direction.

At the summit of the hill, about 1170 feet above the sea, numerous boulders were found, chiefly on slopes facing the N.W.

On the high road to Drumnadrochit and Urquhart, at two or three places a few miles from Affric Hotel, rocks ground down and striated on the south side of the road were noticed. The striæ were running about east and west, or parallel with the axis of the valley.

At the top of the hill, about 660 feet above the sea, several boulders were found, the largest being 4 × 3 × 3 feet. These boulders were resting on a bed of sandy clay, and on a slope of the hill facing west by south. The west side of the boulders was well rounded, as if ground down and smoothed by the friction of bodies passing over it from the west.

All the rocks exposed on the hills here, up to the summit level of the road, which reached about 927 feet above the sea, showed smoothings on their west sides.

The whole of Glen Urquhart has evidently, at some former period, been choked with drift. Beds of gravel, clay, and sand still remain on the hills on each side up to the very top. Hence, probably, the luxuriance of vegetation which this beautiful glen manifests.

On the north bank of Loch Ness, about half a mile to the east of Urquhart, a number of conglomerate boulders lie on the hill side. In walking up the hill the Convener counted six, of which the largest was 7 × 5 × 4 feet, from a level of 200 feet to a level of 800 feet above Loch Ness.

The rocks of the hill here are gneiss, so that these boulders have been brought to where they now lie, most probably from Mealfourvounie, which consists entirely of conglomerate rock, and is situated a few miles to the west.

One of the boulders is at a height of 340 feet above Loch Ness, which corresponds with the line of an old horizontal terrace, visible along the south bank of Loch Ness to the eastward.

At the height of 450 feet above the loch, deep beds of a fine sandy clay occur, just above the landing pier at Urquhart.

XVII.—FORT AUGUSTUS.

On the Corryarrick road (about two miles S.W. of the town) one boulder was noticed which seemed to indicate the direction in which it had come. Fig. 46 shows this boulder of grey gneiss lying on a steep bank of gravel at the base of a rocky cliff, which is a buff-coloured felspathic rock. The slope of the hill is towards N.W. The boulder, therefore, probably came from that direction. It happens to be at the same height above Loch Ness (viz., 207 feet) as the lowest of the conglomerate boulders above mentioned seen to the east of Urquhart.

XVIII.—BEN NEVIS.

The track commonly followed by tourists ascending the mountain leads up the N.W. shoulder of the hill. Boulders of enormous size occur on each side of the track. The following measurements will give some idea of the size of these masses; they happened to be within from 20 to 30 yards of the track; but larger boulders were seen at a greater distance: A boulder $16 \times 10 \times 10$ feet, partially sunk in a gravel bed; a boulder $15 \times 7 \times 5$ feet, lying on rock; a boulder $13 \times 7 \times 4$ feet; a boulder nearly cubical, the sides being about 4 feet square. The three first mentioned had their longer axis N.W. and S.E.; and this was the rule with almost all the boulders, whose length was much greater than their breadth. The boulders measured were at levels above the sea between 900 and 1200 feet. But there were boulders of great size up to 2000 feet or more, and there were some near the base of the mountain. Many of these last-named had, however, been utilised for building purposes. Mr Doig, builder in Fort-William, who accompanied the Convener in his ascent, mentioned, that having been contractor for the Town Hospital, he had made use of one boulder, situated at the foot of the hill, which was four times as large as any of those above mentioned, and that all the rubble-work of the front wall of the hospital—extending to about 80 yards—had been obtained out of this boulder.

Mr Doig, who evidently was intimately acquainted with both boulders and rocks on Ben Nevis, had no doubt that all the boulders on the N.W. shoulder of the Ben were different from any rock in the mountain. He stated that the boulders were mostly all granite, both red and grey granite, but mostly grey. Those examined by the Convener were all grey granite, very similar to the rock worked at Ballachulish and Duror, about 30 miles to the west.

XIX.—SKYE.

The Convener regrets not having had an opportunity of visiting Skye, except at one spot on the west coast, viz., Loch Scavaig, where the steamer stops for an hour to allow tourists to visit Coruisk. Dr Macculloch's book, published in 1818, and the paper which the late Principal Forbes read in this Society on the Cuchullin hills ("Edin.

New Phil. Journal" for 1846) show that in different parts of the island there are boulders and smoothed rocks well deserving of careful study.

After what Principal Forbes said about the existence of smoothed rocks, and of grooves or striæ on these rocks (which he unhesitatingly ascribed to glacier action), it is impossible to dispute that on this island, small as it is, there must have been ice enough in the different corries to form glaciers. Perhaps there would be less difficulty in adopting the theory, were it supposed that Skye had stood much higher out of the sea at the time when these effects were produced.

Principal Forbes in his paper, among other effects ascribed by him to the Skye glaciers, speaks of "the occurrence of large angular detached masses of hypersthene *rock poised upon others*, or *fantastically balanced on the insulated tops of the elliptical domes of rock*" (page 92). He also, on this point, quotes Dr Macculloch, who supposed that these detached masses were merely fragments which had fallen from adjoining hills. But he admits that "the mode in which these fragments lie is remarkable. The bottom of the valley is covered with rocky eminences, of which the summits are not only bare, but often very narrow, while their declivities are steep and sometimes perpendicular. Upon these rocks the fragments lie, and in positions so extraordinary, that it is scarcely possible to conceive how they have risen so high after the rebound, or how they have remained balanced on the very verge of a precipice. One weighing about 10 tons has become a rocking stone. Another of not less than 50 tons stands on the narrow edge of a rock 100 feet higher than the ground below, which must first have met it in the descent" ("Western Islands," vol. i. p. 388).

One of these boulders, perched "on the narrow edge of a rock," was noticed by the Convener near where the boat takes passengers ashore at the head of Loch Scavaig. Fig. 48 represents this boulder—*a* shows its position relative to Lake Coruisk and the sea; *b* shows its position more exactly on the rock where it stands.

Dr Macculloch's idea of the boulder having fallen from an adjoining cliff, and *rebounded* on to the top of the rock where it stands, of course cannot be entertained.

On the other hand, if the boulder was brought by a glacier from

the eastward, and projected from the glacier's surface, would the boulder have rested where it fell? Is it not probable that it would have slid down the smooth rock into the sea?

The surface on which it lies, slopes steeply towards the sea, in a direction W. by N.; and under its S.E. end, there are two small boulders which seem to have obstructed progress in that direction. These circumstances conveyed to the Convener's mind the impression that the boulder may have been brought by floating ice, and been thus landed on the rock which it occupies.

It is right to add that the smoothed rocks, which occur near the shore adjoining the lake, have all the appearance of a great amphitheatre, into which floating ice may have entered, and in which ice may have circulated as in an eddy, abrading the rocks forming the bottom and sides of the amphitheatre.

This view of the matter is not inconsistent with the theory, that before the land was submerged, a glacier had existed in the valley, and formed smoothings and groovings also on the rocks as observed by Principal Forbes.

The Convener, seeing the importance of ascertaining beyond all doubt the true character of the materials forming the site of the "Big Boulder," in Barra (p. 122), wrote lately to Dr MacGillivray of Eoligarry, the tenant of the farm on which the boulder is situated, to request that he would dig under the boulder as far as could be done with safety, and send a written report of what was found. Since these sheets were printed, the Convener has received a letter, from which the following are extracts:—

"Having at length got milder weather, we proceeded to the 'Big Boulder of the Glen,' and made the cuts or drains under it, as you directed, to the depth of three feet on both sides, and also at the west end of the boulder.

"The first substance found for about a foot deep, was black soil or earth and cockle-shells, mixed up with a few stones. Below that, as deep as we could conveniently go, very hard gravel and lumps of stone, extremely firm and difficult to pick out,—I should say, because being so much compressed by the enormous weight of the boulder.

"The rock of the hill did not appear at all on any side, or under the boulder for three feet at least. It seemed resting entirely on soil and gravel; site very high, almost on the surface, so that a spade can be pushed nearly to the centre in one or two places.

"The stone, to even an ordinary observer, would appear to have been brought to its present situation by some agency or other, as the place looks quite unnatural to it."

Notes by William Jolly, Esq., Inverness, on the Transportation of Rocks found on the South Shores of the Moray Firth.

(*Sent to Boulder Committee, October* 1878.)

Along the south shores of the inner portion of the Moray Firth, certain movements of rocks have taken place in geological times which are interesting as bearing on the inquiry into the general transportation of boulders over Scotland. These rocks are, happily, of very distinctive varieties, which renders the question of their source and movements a comparatively easy one. On these, I beg to offer some rapid notes, in connection with the work of the Boulder Committee.

I.—THE GRANITE OF THE DIRRIE MORE.

At the back of Ben Wyvis, on the road to Ullapool, between the Ben and Strath Vaich, there exists a development of a peculiar granite *in situ*, easily seen in passing along the road. The granite occupies a considerable area in the centre of the valley, and is seen in great extent in the bed of the river, to which it imparts a wild and picturesque character, as the water dashes and foams amongst its projecting masses. The rock consists of the usual ingredients of trinary granite, but its distinctive feature is the existence of lenticular pieces of dark mica, arranged throughout its pinkish mass in pretty regular layers, which give the rock somewhat of the general aspect of a stratified deposit. It is peculiar in general appearance, and is easily distinguished wherever seen by its *kenspeckle* character, even when not broken up. This rock is found scattered all over the country to the eastward of its parent position, and would seem to have been carried down the Blackwater valley in which it is found, and also right through the deep glen which exists in the very centre of the great bulk of Ben Wyvis, and which forms its most distinctive feature as seen from the Dirrie More, or Great Slope, as the long road to Ullapool is called. Thus viewed, Ben Wyvis seems cleft into two mighty masses by this great gorge, and has from this point, perhaps, its grandest and most commanding aspect. This granite is found scattered abundantly all over the Black Isle, where it exists as the most abundant surface rock, being imbedded in the debris and boulder clay that clothes the whole of

its surface. It may be seen in boulders which have been broken up for fencing purposes, showing the interior composition very well, along the road from Conan village to Ferintosh, where it forms the greater part of the dyke that skirts the highway. It occurs right on the summit of the Mulbuie, or Yellow Ridge, which forms the backbone of the Black Isle. It can be seen there to good advantage, along the old road between Dingwall and Inverness, which ran right over the Mulbuie between Conan and Tor Inn, a path which must now be traversed on foot, and which commands a magnificent prospect. These granite blocks are scattered all over the eastern slopes of the Mulbuie, and may be seen on the Black Isle coast of the Moray Firth, as at Avoch, Fortrose, and along the district traversed by the high road leading to Cromarty.

The blocks have been carried across, not only the ridge of the Black Isle, but what is now the Moray Firth, to beyond Elgin, and they may be seen on the coast between Burghead and Lossiemouth. At Lossiemouth, on the high ridge of Stotfield above Branderburgh, several masses may be observed in the dyke above the Public School. I have no notes of its appearance east of this point.

II.—THE LOCH NESS GRANITE.

At the northern end of Loch Ness, on its western side, a large patch of red granite exists along the shore—from a point a little south of Loch End Hotel, at a burn just opposite Dores, to a point about a mile south of the mouth of the Abriachan Burn—and extends westwards from the loch in a triangular outline some two or more miles broad, forming the mass of the high hill between Loch End and Abriachan, which there bounds the loch. This granite is fine-grained and of a light pinkish colour, and is used for commercial purposes, numerous examples of it being to be seen in Tom-na-Hurich cemetery near Inverness, and elsewhere. The smallness and compactness of its component ingredients are its chief peculiarities. It occurs in the abundant gravel deposits to the eastwards of Loch Ness, in Tom-na-Hurich for instance, as noted long ago by George Anderson, the eminent geologist and joint author of Anderson's excellent guide-books to the Highlands; and eastwards of this, on beyond Nairn and Forres. It is found less in large boulders, though it occurs in considerable masses, than as forming part of

the gravel deposits which form so marked a feature on the south shores of the Moray Firth.

III.—THE LIVER-COLOURED CONGLOMERATE.

On the east shore of Loch Ness, opposite this granite, extending from Loch Ashie to a little south of the Fall of Foyers, stretches a high ridge formed of Old Red conglomerate, of which also the great mass of Mealfourvounie on the opposite shores of Loch Ness wholly consists, up to its very summit (3060 feet), which is the highest point attained by this basal deposit of the Old Red of Scotland. This conglomerate, on the east side of the loch, is best seen on the Stratherrick road from Inverness, where it runs above Loch Duntelchaig, south of its junction with the road to Dores, and along the side of Loch Kecklish (Ceo-glash), which lies between Loch Duntelchaig and Bochrubin. Here it forms a series of very striking precipices, vertical, bare, and cracked, overhanging the road and loch, and having a remarkable appearance, arising from their form and composition. This conglomerate happens to contain in great abundance, imbedded in its matrix, a certain dark-purplish or liver-coloured quartzite, in pieces of considerable size. This quartzite is so marked and peculiar that it can be easily distinguished in any boulders in which it occurs, and it seems, so far as I have seen (and I have examined the most of the country minutely), to be peculiar to the conglomerate on this part of Loch Ness; so that its existence in any conglomerate block is a very sure evidence of its parent site. A very good place to see it *in situ* is an abrupt little hill close by the junction of the Stratherrick and Dores roads, crowned by an ancient hill fort, called *Caisteal-an-Duin-Riabhaich*, or the Castle of the Grey Hillock, with rough enclosing walls, easily noted from the highway. The fort is also worth visiting, on its own account, and for the fine view obtained from its summit; and there this liver-coloured quartzite may be well seen embedded in the conglomerate which forms the mass of the hill.

This special conglomerate is scattered to the N.E. of this point, in very numerous masses, onwards beyond Elgin. One peculiarity of this rock is that it is found so frequently in large blocks, often of immense size,—so large that they have attracted the attention of the old inhabitants and have received local names; and

they not seldom occupy conspicuous and elevated positions. They are very abundant, on the flat Old Red Sandstone ridge of the Leys, lying between the valleys of the Ness and the Nairn, where they are frequently very large. The great boulder near the battle-field of Culloden, known as Cumberland's Stone, is formed of it, being rubbed, rounded, and grooved on the upper side; the splendid angular, cubical mass of Tom-Reoch, on the opposite bank of the Nairn near Cantray Doon, one of the largest and finest blocks in this part of the country, is a worthy specimen; several large boulders in the fine woods of Cawdor, one of which, the Grey Stone, stands on the edge of the river, near the junction of the two streams that form the burn of Cawdor, a little above the castle, are composed of it: while, east of this, it is represented by exceedingly numerous blocks, the chief of which are *Clach-an-oidhe*, or Stone of the Virgin, 20 feet × 15 × 9, close by the Public School of Geddes; another, near the top of the Hill of Urchany, at a height of 580 feet, called *Clach-na-Calliach*, or Stone of the Old Woman; the fine boulder right on the crest of the hill, a little to the east of this, called *Clach-nan-Gillean*, or Stone of the Boys, at a height of 690 feet; several big blocks on the high ground of Moyness, one in particular lying close by the roadside below the U.P. Church of Moyness or Boghole; the splendid block on the high ridge on which stands the picturesque ruin of Burgie Castle, east of Forres, a short distance beyond the castle, called the *Douping Stane*, from a burgess ceremony performed on it, as lying on the extremity of the town lands of Forres; and a very large mass, still partly imbedded, on the crest of the hill of Roseisle, perhaps of the same rock. Examples of it may be seen on the south shore of the loch of Spynie, not far from the castle, between Elgin and Lossiemouth, The whole country between Loch Ness and Lossiemouth is literally strewed with pieces of this easily distinguished conglomerate. Several of the larger specimens of it have already been visited, described, and figured by Dr Milne Home, in former reports of the Boulder Committee.

IV.—THE GRANITE OF STRATHERRICK.

In the elevated hollow strath, or plateau, known as Stratherrick, which runs parallel to Loch Ness on its eastern side, occurs a large

patch of grey granite, occupying nearly the centre of the strath. It is very well exhibited near the Roman Catholic chapel, where it occurs *in situ* in large masses, and where it has been worked. It is a granite of very good quality, and has been greatly used for building; and it would be much more used if it were more accessible from lines of public communication. It extends down the pass of Inverfarigaig, where it forms the rock of its upper portion, the lower being the Old Red conglomerate. This grey granite is found in blocks of different sizes, some of them large, all over the country east towards Elgin, intermingled with the conglomerate just mentioned; but it never occurs in such large masses as the conglomerate, which, from its nature and original position, it could not do. It is remarkable that this granite is also found in blocks scattered *over the very top of the ridge of conglomerate between Loch Kecklish and Loch Ness*, already described, sometimes finely perched on its very summits, which range between 1400 and 1500 feet, and I have numerous notes of big boulders of it found there.

V.—THE GNEISS OF STRATHERRICK AND THE MONAGHLEA MOUNTAINS.

Parallel to the line of conglomerate blocks scattered between Loch Ness and Lossiemouth, often intermingled with it and the granite of Stratherrick, but occurring much more abundantly to the east of it, is found a broad band of boulders of grey gneiss. These are of all sizes, frequently large enough to have claimed popular notice and to have received local names, and are often placed in remarkable and elevated positions. The character of the rock may be well seen on the side of the road between Inverness and Farr, in the dyke near the Free Church of Farr, and in the fine group of boulders in the centre of the valley, which forms so striking and interesting a geological feature there. They occur in astonishing numbers round *Loch-na-Clachan*, or the Loch of the Stones, into which the stream from Loch Duntelchaig flows, near the old parish church of Dunlichity. There they form grand and picturesque groups of all sizes and forms, on the east side of the loch and up to the elevated summits of the hills, above 1400 feet high, and where they may often be seen, right on their crests, standing in a serrated line against the sky. Altogether, this is one of the most remarkable aggregations of blocks that I know, and it has already been referred

to by Dr Milne Home, in his valuable paper on Glen Roy. Farther up the Nairn, near Farr House, stretches a long flat plateau of gravel and other debris, which stretches right across the valley, and through which the river has had to cleave its way in the narrow gorge below Flichity Castle. On this plateau is found another striking and numerous assemblage of huge blocks, well worth a visit, often of large size and peculiar forms, scattered singly and in groups, some of them standing erect like great pillars. Frequently these gneiss blocks have been left in remarkable places. On Craig-a-Chlachan, which overlooks the church of Dunlichity, on the west shore of Loch-na-Chlachan, near its top, on the edge of a steep precipice, is poised a block of gneiss 14 feet long, 10 feet in height, which catches the eye of the traveller from all points, and is known as *Clach-na-Fhreiceadan* or *Faire*, or the Stone of the Watch, on account of its elevated station (1120 feet), standing, as it does, like a sentinel, to guard the surrounding region.

To the east of the Free Church of Farr, right on the peaked top of the highest hill seen from that part of the valley, may be observed what seems a shepherd's cairn marking its summit. This provoked my curiosity for years, and this season I ascended the mountain and found that it consisted of a great block of gneiss split in two, and known, from this circumstance, as the *Clach Sgiolte*, or Split Rock. It has been originally a cube of stone, 9 feet square and 5 feet high, now split at two-thirds of its breadth, the larger part having remained in its original position and the smaller having fallen over. It stands nearly 1000 feet above the valley below, and nearly 1600 feet above the sea. Another *Clach Sgiolte*, on or very near the top of the great mountain, overlooking the narrow gorge of Conaglen, near Dunmaglass, at the very head waters of the Nairn, called Ben Dhu Choire, at a height of 2260 feet. This block I have not yet ascended to.*

Another striking example of these gneiss blocks is found beyond the inn of Flichity above Farr, on the north slope of the finely crested ridge that lies between the valley of the Nairn and Loch Ruthven. It is called *Clach-a-Bhonat*, or the Stone of the Bonnet. This is a very large block, worth a visit. In this part of the valley of the Nairn, numerous other blocks occur singly and in groups in

* There is another *Clach Sgiolte*, about $1\frac{1}{4}$ mile from the source of the Findhorn, called the Eskin, some 2070 feet above the sea.

the bottom of the valley, and high on its sides up to the crests of the enclosing hills, on which they may be seen standing against the sky line.

Farther down the valley, below Daviot and not far from the mansion of Nairnside, a very fine boulder is perched on the top of a steep rock overlooking the river, on its eastern bank. It is 21 feet × 12 feet × 15 feet in height, and forms a fine object as seen from below, from the peculiarity of its position and great size. It is called *Clach-an-ullaidh*, or the Stone of the Treasure-Trove, from the prevalent idea that treasures lie concealed under such remarkable rocks; for there are numerous blocks with the same name and tradition, in various parts of the Highlands.

On the same side of the Nairn, and not far from the block just mentioned, another is found, high up on the hill bounding the valley, and seen against the sky from below, very distinctly so from the Cumberland Stone, and from the road to the far-famed Clava, with its cairns and standing-stones. It is called *Clach-a-nid*, differently interpreted to be the Stone of the Nest, an unlikely meaning, and more probably the Stone of the Whistle, as the point to which the herd ascended to whistle and call on the cattle scattered over the hill slopes there, when he went to drive them home for the night. It is a very fine block, measuring 21 feet × 21 feet × 20 feet high, and has a commanding position (950 feet), with a splendid prospect, over the pastoral Nairn, away to the distant N.W. Highlands.

There are numerous other blocks of the same gneiss worthy of mention, but the foregoing will suffice as examples. They are found extending to the eastwards like the rocks already mentioned.

VI.—THE KINSTEARY GRANITE.

Near Nairn, on the estate of Kinsteary, occurs a considerable development of granite, of distinctive character and great value. It is of a rich flesh-colour, and its chief feature is the existence of fine large crystals of orthoclase felspar, which give it its special beauty, approaching in appearance as it does to a rich-coloured marble. It has only been recently worked for the market, but has already taken a high place, and is largely used in London and elsewhere for fine ornamental purposes. It may be seen *in situ*, quarried at different places, at a short distance from Nairn, on the road to Ardclach.

This peculiar granite, which can easily be distinguished wherever it occurs, is found abundantly to the eastwards of its original position. In all the dykes and houses in and round Auldearn, and all over the Moyness district, it may be seen as the most abundant rock. It extends eastwards beyond Forres, gradually lessening in amount but still abundant, over the flats of Kinloss and up on the high ridge of Burgie to its summit above the *Douping Stone*, and beyond Elgin to Lossiemouth and further east. Pieces of it may be seen on the shores of Loch Spynie near the blocks of conglomerate already mentioned.*

The foregoing are the chief examples of travelled boulders found on the south shores of the Moray Firth. Many others occur, but these have been mentioned because they consist of rocks of a more or less pronounced character, easily distinguished where seen; therefore furnishing important evidence as to the direction and extent of the transporting agents. From the map, it will be seen that the general direction of movement of these blocks has been eastwards, but chiefly from S.W. to N.E., parallel to the trend of the coast of the Moray Firth at this part. None of these rocks are found to the *west* of the points *in situ* where the parent rock is found; at least, I have found none, and I speak from a pretty extensive knowledge of the district. What the transporting agent or agents were—whether glaciers, or icebergs, or ice-floes, or water currents, or one or more of these together—however interesting and important—it would be foreign to the purpose of the present paper to consider; but that these rocks were carried from their native sources and scattered widely and numerously to the eastwards, over a large extent of country, cannot for a moment be doubted. †

WILLIAM JOLLY,
H.M. Inspector of Schools, Inverness.

* I have no notes of the distribution of these boulders east of Lossiemouth. Mr Wallace, head master of Inverness High School, and a good geologist, tells me that he saw recently large blocks of both the Dirrie More and Kinsteary granites at Buckie in Banff, dug out of the new harbour. It would be interesting to ascertain how far east these easily distinguished rocks have been carried.

† The author purposes entering into greater detail in regard particularly to the remarkable carried blocks of the valley of the Nairn, in a special paper on the glaciation of that valley.

I.

Observations on Boulders and Drift on the Pentland Hills.

By Alex. Somervail, Stationer, Edinburgh.

Besides the boulders described by the late Mr Charles Maclaren in his "Geology of Fife and the Lothians," and also by Professor Geikie in the "Edinburgh Memoir of the Geological Survey," as having been carried from the Highlands, there are others which would indicate a transport from a different direction.

On the highest summits of the Pentland Hills (Scald Law, Carnethy, South Black Hill, North Black Hill, and others which are composed of various varieties of porphyrites) are found numerous boulders of fine conglomerates, grits, and sandstones, intermingled with a few boulders of quartz, greenstone, and other rocks, all partially or entirely covered by a deposit of peat, which in some places on and near the summits of the hills attains a thickness of nearly six feet. The sandstone boulders vary in size from mere fragments up to large masses which I was unable to dig up. They are common on the very highest point of Carnethy (S. 1),* more so on Scald Law (the highest of the range) and South Black Hill, and still more abundant on the West or North Black Hill. They are smaller in size and less numerous as we approach the hills in the neighbourhood of Edinburgh—viz., towards the east.

On a careful examination of the above-mentioned sandstone boulders, with regard to mineral composition, texture, and colour, there can be no doubt that they have been derived from the sandstone strata which form the Cairn Hills. The highest point of the Cairns is 1844 feet, or 46 feet below the level of Carnethy, and 54 feet lower than Scald Law, which is 1898 feet above the sea-level.

It would follow from this, that the transport of the sandstone boulders has taken place from S.W. to N.E., or very near this direction. There are other indications which confirm this movement. Mr John Henderson has, in the "Transactions of the Edinburgh Geological Society," vol. ii. page 365, described the occurrence of a

* A plan of a portion of the Pentland Hills, to illustrate Mr Somervail's and Mr Henderson's notes, is appended. On this plan the localities mentioned by Mr Somervail and Mr Henderson are indicated by the letters S. and H. respectively.

large slab of sandstone lying in the gorge of the Bonally Burn, derived from beds of the same rock about half a mile to the S.W.

In a deep cutting recently made in the boulder clay at Alnwick Hill, near Liberton, I observed many boulders of Old Red Sandstone, which must have been carried from the vicinity of the Carlops, where the same rock occurs *in situ.* There were also boulders of various varieties of porphyrites, which form the hills to the S.W. The same remarks hold good with regard to boulders I saw dug from very deep excavations made two years ago at Seafield, near Leith, all bearing out a transport of some kind along the trend of the Pentlands, or from S.W. to N.E.

A fact in connection with the Old Red Sandstone boulders I observed at Alnwick Hill appears worth recording. Many of these boulders were very round and smooth, so much so that they suggested the idea that the agent which transported them to their present position could not have produced this effect during transport, as the distance from their source is so very small, but in all likelihood found them worn and rounded before being carried along.

NOTE ON THE BOULDER CLAY.

There is, in my opinion, no true till or boulder clay resting on any of the Pentland summits. What has been described as such by Dr Croll on the top of Allermuir Hill seemed to me simply a peaty soil formed by the decomposition of the underlying rock and debris, and the decay of vegetable matter, making up a heterogeneous deposit which, however, has no connection with the true boulder-clay occurring at lower levels.

II.

Notes on Drift and Glacial Phenomena on the Pentland Hills.

By John Henderson, Curator of the Phrenological Museum, Edinburgh.

The following phenomena were noted by me during a number of visits I made to the Pentland Hills; but as my chief object then was to examine the older rocks, my observation on the recent deposits are by no means complete.

Striated Rock Surfaces.—Only two localities were observed. Dr Croll, in a paper on "The Boulder Clay of Caithness," first made known that he had discovered a striated rock on the top of Allermuir Hill, at a height of 1647 feet above the sea. I visited Allermuir Hill some time after this discovery, and was fortunate enough to find a portion of the striated surface. The rock of the summit of the hill is felstone, very much weathered and broken up, and it is only in the little hollows, which are covered with a blackish earth, that indications of rubbed or scratched surfaces are found. The portion I discovered, although it was only a few inches square, was finely striated, and I had no difficulty in making out the direction of the striæ, and the direction from which the striating agent had come, which was about W.S.W. (See Plan, H. 1.) The other locality I discovered during a very dry summer, when the water in Bonally Pond was very low. The striæ occur here on a reddish sandstone, which crops out along the south-east side of the pond, at an elevation of about 1100 feet above the sea (H. 2). There is here a much larger surface of striated rock than on Allermuir Hill, but it is mostly always covered by the water of the pond. I had an opportunity, however, of seeing a portion of it again last summer. I then took the direction of the striations, and found them, as on Allermuir Hill, W.S.W. I may remark that this agrees with the direction of the striæ on the rocks of at least twenty localities that I have examined in the neighbourhood of Edinburgh, and in no instance in this neighbourhood have I observed the rocks striated in a direction N.W. and S.E.

Boulders occur at all heights up to 1400 feet, and all sizes up to 10 or 12 tons. Several very large ones lie on the north side of Capelaw Hill, at about 1200 feet above the sea (H. 3). They are of a dark crystalline greenstone, unlike any of the igneous rocks in this district. Further west, on the west side of Harbour Hill, there is a great number of smaller blocks of the same greenstone (H. 4). They appear to me to lie on about a uniform level along the hill side, at about 900 or 1000 feet above the sea. The prevailing boulders in the northern portion of the hills are of greenstone, while those further to the S.W. are mostly sandstone.

Boulder Clays.—I have observed two localities where these occur in considerable quantities, one at the north-west corner of Glencorse

Reservoir, at an elevation of 900 feet (H 5). It is a stiff reddish clay, full of well rubbed and scratched stones, and differing in no way from the boulder clay of the lower districts. The other locality is about three miles to the S.W. of this, in the same line of valley between the hills, at an elevation of about 1100 feet. It is of the same character as the last, but is covered by a great deposit of gravel and boulders, which extends across the broad valley between Hare Hill and South Black Hill (H. 6).

Another large deposit of gravel and boulders is at the mouth of the broad valley in which the Bonally Pond lies, at an elevation also of 1100 feet above the sea. This deposit encloses some very large boulders of greenstone (H. 6).

III.

The Convener appends to the foregoing Notes by Messrs Somervail and Henderson, the References by the late Charles Maclaren, by Professor Geikie, and Mr Jas. Croll, to Striæ and Boulders on the Pentlands, as the localities are embraced in the same map.

1. Mr M'Laren, in his "Geology of Fife and the Lothians," states :—

(1.) "There are few opportunities of observing 'groovings' on the Pentland Hills. I noticed them, however, at Westwater of Dunsyre, on the top of a thick bed of hard sandstone, from which 12 or 14 feet of alluvium had been removed. The dressings pointed exactly east and west; and the evidence was the more satisfactory, as the direction of the stream on whose bank the rock was situated, and of the valley in which the stream flowed, was south and north. They were very distinct, the larger groovings being about $1\frac{1}{2}$ inch broad, and $\frac{3}{10}$ths of an inch deep. The locality must be 800 or 900 feet above the sea" (page 294).

(2.) Travelled blocks are important in two respects:—*first*, as indicating the action of currents or other transporting agents no longer operating; and *next*, as illustrating changes which have taken place subsequently to their deposition in the spots where we find them.

a. "In the Pentlands there is a boulder of mica slate, weighing 8 or 10 tons, on the east end of Hare Hill (see plan annexed, M. 1). It reposes on the surface of the west side of the glen leading north from Habbies How to Bavelaw, on a declivity about 80 feet above the bottom. The nearest spot from which this mass could be derived is the portion of the Grampians about Loch Vennacher or Loch Earn, 50 miles distant" (page 301).

Further, this block tells us that the surface of the hills where it now rests must have been in a different condition when it was deposited. It lies on the side of a declivity, where a large stone, either hurried hither by a current, or dropped from an iceberg, would not stop, but roll down to the bottom of the valley. The reasonable inference is, that the valley between Hare Hill and North Black Hill was then filled with "materials which have since been washed away" (p. 302).

b. Half a mile south from this, three greenstone boulders of 2 or 3 tons weight each are lying on the edge of a precipice, about 200 feet above South Burn. (See plan, M. 2.)

These have certainly travelled some miles, and the bed of clay seen below them is no doubt a remnant of that which then filled up the ravine, and prevented them descending to the bottom.

c. On the east end of West North Black Hill there is a sandstone boulder of 8 tons weight. (See plan, M 3.)

This block may not have travelled far. But it rests on a surface as steep as the roof of a house (inclined both above and below at 45°), and about 400 feet above the bottom of the valley.

It is impossible that it could be dropped here, or brought to the spot by a current, without descending to the bottom, unless sustained in its place by matter since removed.

This single block informs us that the ravine about 100 yards wide at the surface of the marsh, which separates Black Hill from Beild Hill, must then have been filled up with alluvial matter to the height of 400 feet at least above its present bottom, which is probably 50 feet above the true bottom in the rock (page 302).

d. On the south declivity of Harbour Hill, about 300 feet above the level of the Compensation Pond, there is a very large boulder of greenstone weighing 12 or 14 tons. (See plan, M. 4.)

The surface it rests on is not steep. But the boulder must have

travelled many miles; for there is no greenstone of the kind in the hills, and none near them, except in situations 500 or 600 feet lower.

This block has probably been transported in the same manner as the mass of mica slate (*a* above).

e. The same remarks apply to a greenstone boulder lying half a mile N.W. of Logan House, on the south side of West Black Hill, about 1400 feet above the sea. It is of 12 or 14 tons weight. (See plan, M. 5.)

There are many others in elevated situations of 3 or 4 tons weight.

The substance is generally greenstone, the least brittle probably of all rocks, and of course the best fitted to resist fracture. Nearly all the blocks have their angles rounded off.

f. On the banks of Eight Mile Burn, in the low ground, there is a mass of alluvium about 100 feet thick, containing hundreds of trap boulders of all sizes up to 10 tons weight. It consists of two beds,—the older, a blue unctuous clay, the newer a red clay. The large blocks are chiefly in the latter.

There are many similar travelled blocks in the burn flowing from the old Reservoir to Bonally, and probably in all the streams of these hills (page 303).

2. Professor Geikie, in his Memoir "On the Geology of the Neighbourhood of Edinburgh," published in 1861, observes (1) that "boulder-clay lies along the north-west flanks of the Pentlands, rising to a level of at least 1300 feet."

When the clay has been recently removed, we usually find the rock below polished, grooved, and scratched in a direction nearly E. and W., or E.S.E. and W.N.W. These markings even remain distinct on hard greenstones which have remained exposed to the weather for an indefinite period.

The parallelism of the striations throughout the present district shows that the floating ice must have moved in a pretty uniform direction; and that it was from the west is rendered clear by the striation of the western face of the hills, by the great depth of drift on their eastern sides, and by the fact that the transported boulders, when traceable to their parent rock, have been carried from west to east (page 126).

(2.) Of boulders which have undoubtedly been transported either from Cantyre or the Grampian Highlands, I may refer to the mass of mica slate about 8 or 10 tons, on the S.E. side of Hare Hill above Habbie's How, which was first noticed by Mr Maclaren.

(3.) On the other side of the valley, on the S.W. slope of North Black Hill, several smaller masses of white quartz rock occur, fully 1300 feet above the sea-level.

Masses of gneiss, mica-slate, and a hard metamorphic conglomerate, are found in tolerable abundance all over the district.

3. Mr Croll, in "Climate and Time," gives the following observations:—

"On ascending *Allermuir Hill* (1617 feet), Mr Bennie and I found its summit ice-worn and striated. The striae were all in one uniform direction, nearly east and west. On examining them with a lens, we had no difficulty in determining that the ice which affected them came from the west, not from the east. On the summit of the hill we also found patches of boulder clay in hollow basins of the rock. At one spot it was upwards of a foot in depth, and rested on the ice-polished surface. Of 100 pebbles collected from the clay, just as they turned up, every one, with the exception of 3 or 4 composed of hard quartz, presented a flattened and ice-worn surface, and 44 were distinctly striated. A number of these stones must have come from the Highlands to the north-west.

"On ascending *Scald Law* (1808 feet), 4 miles S.W. of *Allermuir*, we found in the debris covering its summit hundreds of transported stones of all sizes, from 1 to 18 inches in diameter" (pp. 441, 442).

2. Remarks on the Boulder Report by the Convener of the Committee, read (in the absence of Mr Milne Home, Convener), by Mr Ralph Richardson, Member of Committee.

This Report contains information applicable to three districts of country, namely—

1. Pentland Hills.
2. Morayshire.
3. Islands of the West Coast, and part of the Mainland.

1. Pentland Hills.

The impression hitherto had been, that the boulders on these hills indicated a movement exclusively from the north-west; and there is no doubt that the mica slate boulders on these hills indicate such a direction; but Messrs Somervail & Henderson, in the notes contained in this Report, have discovered a separate movement from the west-south-west, by the occurrence of certain sandstone blocks, which they think can be traced to a particular hill or hills in the Pentland range. This point is so important, that it is hoped further inquiry may be made regarding it.

2. Morayshire.

The boulders in this country are described in a very interesting Report by Mr Jolly, Inverness, a member of the Committee. Mr Jolly's concluding paragraph deserves notice. He says, "None of these boulders are to the *west* of the points *in situ* where the parent rock is found—at least I have found none, and I speak from a pretty extensive knowledge of the district. What the transporting agent or agents were, whether glaciers, icebergs, ice-floes, or water-currents, or one or more of these together, however interesting and important, it would be foreign to the purpose of the present paper to consider; but that these rocks were carried from their native sources, and scattered widely and numerously to the *eastwards*, over a large extent of country, cannot for a moment be doubted."

3. Islands of the West Coast, and part of the Mainland.

It will be seen from the Report that my own personal survey last summer was chiefly among the islands, which, commencing with Iona at the south, stretches through the Western Hebrides to the north end of the Lewis, a distance of 120 or 130 miles. I selected these islands for two reasons:—1st. Because the boulders on them would be in their original undisturbed positions; 2d. Because one of the agencies by which transport of boulders has hitherto been most commonly explained, *i.e.*, local glaciers, could hardly be adopted for these island boulders. The highest mountain in any of these islands does not exceed 2000 feet, and on most of the islands the height of the hills does not exceed 500 feet. Moreover, even in the

hilliest districts, there are no valleys in which glaciers could have been formed. Therefore, in studying the question of boulder transport, I thought the problem would be simplified when one of the explanations, and that the most commonly received, was inapplicable. In this view of the matter, I am glad now to find that Mr James Geikie, the well-known persistent advocate of glacier agency, concurs. Since my visit to the Hebrides, I have read a most interesting report by him, published in the "Transactions of the London Geological Society" for October last. In this report Mr Geikie describes a visit he had paid to the Western Hebrides. Referring to South Uist, the highest hill in which (Mount Hecla) reaches to about 2000 feet above the sea, Mr Geikie says—"The nature of the ground is not such as would *favour the formation of local glaciers of any importance.* If such ever hung on the southern slope of the mountain, they must have been of insignificant size, for they have left no moraines behind them."—(Page 842.)

So also, in referring to North Uist, Mr Geikie says—"I saw no trace of terminal moraines. In short, evidence of *local glaciation* appears to be wanting, and if any local glacier ever did exist, it must have been of insignificant dimensions."—(Page 848.)

The question remains whether the Islands suggest any other agency than local glaciers. In examining the Report, it will be found that the following positions seem to be established:—

Boulders.

1st. The great majority of the boulders are situated on the slopes of hills which *face the north-west.*

2d. Though there are boulders in all parts of the Islands, they are more numerous on the *West sides of the Islands* than elsewhere.

3d. Multitudes of boulders occupy *positions* which they could not have come into, except from the north-west.

4th. In a few cases, the *parent rocks of boulders were discovered.* On the mainland, near Gair Loch and Loch Maree, several boulders of a peculiar sandstone rock occur, which must have come from north-west, as it is only in that direction that there are rocks of the same description.

Also on the north end of the Lewis, granite boulders occur traceable to hills situated to the westward.

As to boulders on the west coasts, if they came from the north-west, there can of course be for them no parent rock in this country.

Rock Surfaces.

The Report contains information on other points bearing closely on the question of boulder transport.

I was particularly struck with the fact, that the bare rocks of the Islands, and also on the mainland, were *smooth* on the sides facing the *west*, but *rough* on the sides facing the *east.*

In some parts, the rocks had evidently been ground down under the operation of heavy bodies moving over them. *The direction in which this grinding had taken place* was in many instances shown by long lines of furrows and striæ, which were uniformly in the direction of north-west and south-east.

Mr Geikie, in his first paper on the Hebrides, read to the London Geological Society in 1873, notices one or two of these striated rocks, and admits the direction to be as now stated. He asserts very firmly, that the striating agent *must have moved from the south-east.* It appeared to me, however, on a minute examination of the individual striæ, that the agent which produced them moved from the *north-west,* inasmuch as the striæ or ruts were generally deepest at the north-west ends, and faded away at the south-east ends.

In two cases of these striated rocks, I saw clearly what had been the *tools* which produced the striæ. The rock was being uncovered by work-people for the sake of obtaining road materials. The covering of the rock consisted of a sandy clay, having imbedded in it angular pebbles of quartz, granite, and other hard rocks. If these materials were pushed and pressed over the rock, there could be no doubt what the effect would be.

Submarine Banks.

Another set of phenomena bearing on this subject, is the existence of gravel and clay beds of undoubted submarine formation. I visited a brickwork near Stornoway, the clay of which contains fragments of sea shells, at a height of 250 feet above the sea. I heard of there being similar beds for miles along the coast on both

sides of the Butt of Lewis, but I was unable to visit them. I learn that Mr James Geikie had not only seen these clay beds, but found that some of the shells in them were of an Arctic type.—(See Report, p. 37.)

There are also in the north part of the Lewis, long ridges and high mounds of gravel, sand, and mud, which must have been formed by the action of water; and it deserves notice that some of the longest of these kaims run in a north-west and south-east direction.

In connection with the smoothed rocks, and these submarine formations, it is not unimportant to remark that many of the boulders must have come at a *subsequent* period; for they lie *upon* the striated rocks, and also *upon the kaims and gravel mounds.*

Now, the question is whether, in any part of the world, we find phenomena analogous to the facts brought out in this Report?

In Sir George Nares' account of his recent Arctic voyage, the following account is given by Captain Fielden, the Naturalist of the expedition (vol. ii. page 343):—

"*Sea-ice* moved up and down by tidal action, or driven on shore by gales, was found to be a very *potent agent* in the *glaciation of rocks and pebbles.* The work was seen in progress along the shores of the Polar Basin. At the south end of a small island in Black-cliff Bay, lat. 82° 30′ N., the bottoms of the 'hummocks, some 8 to 15 feet thick, were studded with hard limestone pebbles, which when extracted from the ice were found to be *rounded and scratched* on the exposed surface only.'

"On shelving shores, as the tide recedes, the hummocks, sliding over the subjacent material down to a position of rest, make a well-marked and peculiar sound, resulting from the *grating of included pebbles with the rocky floor beneath*, or in some cases on other pebbles included in drift overlying the rock."

Sir George Nares, on landing on Norman Lockyer's Island, found the low part for some 300 feet above the present sea-level a succession of raised beaches. "The rock is composed of Silurian limestone. On the summit of a hill 900 feet high, *the whole surface of the exposed rock is marked with ice scratchings*, in a north and south direction."—(Vol. i. page 85.)

A case exactly parallel is mentioned by Sir Charles Lyell, who, when travelling in the Bay of Fundy, North America, fell in with

a large surface of flat sandstone rock on the sea-beach, containing striæ and furrows, of which he gives a diagram, and which he was satisfied had been formed by hard stones fixed on the bottom of floating ice, or pushed before it. At the place in question, the tide rises from 40 to 50 feet, and flows at the rate of ten miles an hour, so that the work done when the sea covered the rock, became visible when the tide was out. — ("Travels in North America," by Sir Charles Lyell, vol. ii. page 174.)

In Mr Campbell's "Short American Tramp," several similar cases on the shores of Labrador, of rocks not only ground and smoothed, but also striated, are given (pages 53, 94, and 107). He mentions also the following fact :—"The effect of heavy ice on the water-line is here conspicuous. A berg about 40 feet out of water was aground at the back of one steep island. It seemed to have taken the form of the rocks, against which it was ground by a heavy swell. *The ice was actually rubbing the stone for that height above water, and for* 400 *feet under it.* It was moved by all the power of an Atlantic wave. *Along the whole coast, for a height of from* 40 *to* 50 *feet, an irregular zone of rock is thus scoured bright and smooth.*"—(Page 93.)

I have thought it right to quote these authorities, because of the strong opinion recently expressed, in more than one influential quarter, against the possibility of rocks being ground, smoothed, and striated by floating ice.

One thing is clear, that in the Hebrides the sea must have stood at a higher level at former periods, and that the sea had an Arctic temperature. But on the mainland of Scotland, there are tracts of what appear to be sea-beds of gravel, sand, and clay, up to a height of at least 2000 feet. Reference may be made to Arctic shells on Snowdon, and near Macclesfield, at nearly the same level, in beds of clay and gravel. Along with these facts, it must be remembered that boulders have been found on our Highland hills, even up to 2000 feet, and in some cases upon what are unquestionable submarine deposits. In this respect Scotland presents nothing different from what exists elsewhere. In Norway, Sweden, and North America, there are in like manner boulders lying on what are now admitted to be submarine deposits at very high levels. To this fact Mr Croll, in his highly speculative volume called "Climate and

Time," makes the following reference :—" In every part of the globe where glaciation has been found, evidence of the submergence of the land has also been found along with it." "The submergence of our country would of course have allowed floating ice to pass over it, had there been any to pass over; but submergence would not have produced the ice, neither would it have brought the ice from the Arctic regions, where it had already existed."—(Page 390.)

Of course, there must have been some oceanic current from the north-west sweeping over this part of Europe, and which continued from time to time, as the sea fell, bringing boulders to be deposited from ice-floes on the hills. Where the boulders came from, cannot at present be even conjectured. The fact only is established that, when the boulders came, the sea stood at least 2000 feet above its present level, and that there was a north-west current, with floating ice, which brought the boulders.

It is hoped that this boulder inquiry will continue. There is now more than geological interest attaching to it, for the facts seem to throw light on important astronomical questions connected with the physical condition of our planet. Mr Croll's theory is, that at one period, ice had accumulated at the North Pole to the extent of three or four miles in thickness, which would of itself cause an elevation of the sea, for reasons explained by him, and resting apparently on sound principles.

Before concluding these remarks upon the Hebrides boulders, and upon the evidence they afford of a transport from the north-west, I wish to refer to a confirmation of these views which I have had much pleasure in finding from the last Report of the Boulder Committee of the British Association. In this Report an interesting account is given of individual boulders and groups of boulders in Leicestershire. In most of the cases mentioned, the parent rocks have been discovered, and in all these cases, *these parent rocks are situated to the north-west of the boulders.*

With regard to the general direction of the agency concerned in the transport of boulders, and the smoothing of rocks in the districts embraced in the Report, which is shown to have been from W.N.W., it is important to remark that there are exceptions, and to note the circumstances on which these exceptions occur.

In two parts of the Lewis, where hills prevail, the movement, as indicated by the positions of the boulders, and the smoothed surfaces of rocks, has been from W.S.W. At these places (page 26 of Report, near foot, and page 20, near top,) it will be observed that there are low-lying narrow defiles or elongated valleys, forming grooves through the country, whose general axis is W.S.W., and with rocky sides.

My idea is, that whilst in the *higher* parts of the country the general traces of a W.N.W. current is everywhere distinct, in *low-lying* valleys, the direction of the current would change, in correspondence with the axis of these valleys. Hence, in valleys, the positions of the boulders are often not the same as at higher levels.

This remark probably applies to some of the boulders reported on by Mr Jolly of Inverness. Thus the Derrie More granite boulders, traceable even as far east as Elgin and Lossiemouth, indicate transport from about W.N.W. On the other hand, the boulders, whose parent rocks are situated on the hills forming the sides of the Great Glen (viz., Caledonian Canal), have moved, not E.S.E. (the normal direction), but E.N.E. There can be no doubt that when the sea stood at a height of say 2000 feet above its present level, and with a general oceanic current from the W.N.W., the current in *Glen-na-Albin* itself —*i.e.*, between the walls of the valley—would be in the line of that valley, forming a deep fissure across the country, running about N.E., and that the force of this abnormal current would be sufficient to carry boulders transported through it in a north-easterly direction, till it united with the more general stream from the W.N.W.

Before closing these remarks, I wish to point out, that there are two localities mentioned in the Report which afford evidence that Local Glaciers had existed before the country was submerged. These places are Glencoe and Loch Etive. It is shown in the Report how in the first instance these glens had been scoured out and polished by ice flowing down the valleys, bringing rocks, which exist *in situ* only at the head. It must have been after this period that submergence occurred, because marine deposits of gravel and sand are found in different parts, with boulders resting on them, which undoubtedly came from some distant point in the north-west. These would have been all scoured out by a Local Glacier, had they been deposited at a preceding period.

4. Quaternion Investigations connected with Minding's Theorem. By Professor Tait.

(*Abstract.*)

Minding's Theorem deals with what may be called by analogy the "focal lines," of the system of single resultants of a set of given forces, applied at given points to a rigid body, when these forces are turned about so as to preserve unchanged their inclinations to one another.

Having obtained an exceedingly simple proof of the theorem by quaternions, I next tried to find the locus of the foot of the perpendicular let fall on each of these resultants from the "centre of the plane of centres." The resulting equation is very complex:—but if we extend the data so as to include *every* position of the central axis (whether there is a couple or no), we arrive at a very simple, and at the same time singular, result.

The locus has then the equation

$$\rho = \psi \varpi \psi \alpha,$$

where α is a given vector, ϖ a given pure strain, and ψ *any* rotational strain. This represents a *volume* not a *surface*.

In the statical problem

$$\varpi \alpha = 0,$$

and the locus is the *volume* included between the two sheets of the electric image of a Fresnel's Wave-surface, in which one of the three parameters is infinite. This image has the equation

$$S\,\rho\,(\varpi^2 + \rho^2)^{-1}\rho = 0,$$

a surface whose treatment is easy. But when $\varpi\alpha$ does not vanish we have for the boundary of the locus

$$S\,(\rho - \varpi\alpha)\,(\varpi^2 + \rho^2)^{-1}(\rho - \varpi\alpha) = 0,$$

which is by no means so simple.

Monday, 5th May 1879.

Dr BALFOUR, General Secretary, in the Chair.

The following Communications were read:—

1. The Pituri Poison of Australia. By Dr Thomas R. Fraser, Professor of Materia Medica, University of Edinburgh.

(*Abstract.*)

An opportunity for examining the Pituri of Australia was afforded to the author by Dr Bancroft of Brisbane, who, in 1877, gave him

for that purpose specimens of dry broken leaves of the plant, in the form in which it is used by the natives, and also a small quantity of watery extract.

After describing its use by the natives of Australia, the author advanced reasons in support of Von Mueller's opinion, that Pituri is obtained from *Duboisia Hopwoodii*, and that this plant should be placed in the Solanaceæ.

By a modification of Sta's process for the separation of alkaloids, he obtained from the extract an active principle in the form of a pale, yellowish-brown, alkaline fluid, freely soluble in water, alcohol, ether, chloroform, amylic alcohol and benzole; of a greater specific gravity than that of water; and possessing a burning alkaline taste, and an odour resembling that of both conia and nicotia. A solution in water of this alkaloid and of its hydrochlorate gave precipitates with bichloride of platinum, solution of iodine in iodide of potassium, iodide of mercury and potassium, iodide of cadmium and potassium, iodide of potassium and bismuth, picric acid, bromine water, perchloride of mercury, trichloride of gold, and other re-agents, and several of the precipitates were crystalline. Strong solution of the hydrochlorate gave precipitates with potash and soda.

The chemical characters suggested a close resemblance between pituria and nicotia, which was supported by the examination of the physiological action. The extract and active principle act as local irritants, and produce death mainly by rendering the respiration difficult or impossible. The circulation is, however, also affected, the strength of the cardiac systole being at first increased and afterwards diminished. It was also found that the cardio-inhibitory fibres of the vagus are at first stimulated and then paralysed; that the arterial blood-pressure is at first increased and then greatly diminished; and that, in frogs, the peripheral terminations of the motor nerves are paralysed, and the cutaneous pigment becomes diffused. Contraction of the pupils occurs before death.

When either the extract or active principle is applied directly to the eye-ball, irritation with increased lachrymation is produced, and the pupil becomes for a short time contracted, and afterwards dilated. The last effect was not observed with a specimen of nicotia in the author's possession, and, accordingly, a doubt is

suggested as to the identity of the pituri alkaloid with the alkaloid of tobacco; but it would appear that some observers have noted dilatation of the pupil as a result of the direct application of nicotia to the eye-ball.

It was pointed out as a remarkable fact that the aborigines of Australia should have discovered, and when discovered, placed a high value upon the action of a substance closely resembling in its composition and effects the tobacco so well known to, and so highly appreciated by, millions of the human race.

2. On the Structure and Affinities of the Platisomidæ. By Dr R. H. Traquair.

3. Note on the Probability that a Marriage entered into by a Man above the Age of 40 will be Fruitful. By Thomas Bond Sprague, M.A., F.R.S.E. (Plate XIV.)

When it is desired to disentail a landed estate, it is necessary for the heir in possession, after obtaining the consent of the first substitute heir, to pay to the second and third heirs the estimated value of their expectancy or interest in the estate. In the calculations that have to be made for the purpose of ascertaining this value, the actuary has often to take into account not only the probabilities of life, but the probabilities of marriage and of leaving issue. The heir in possession may be unmarried, in which case he may marry at some future time, and leave a child who would inherit the estate to the exclusion of the subsequent heirs; or the heir in possession may be married but have no children, and the probabilities then to be estimated are (1) that his present wife will have a child at some future time, and (2) that she will die before her husband, that he will then marry a second time, and have issue by his second marriage. Similar contingencies may occur with regard to the first substitute heir. The theory of the calculation of life contingencies is well understood; and much has been done in the way of accumulating marriage statistics and calculating the probability that a man of any age, bachelor or widower, will marry at some future time; but little, if anything, has been

done in the direction of estimating whether the marriage so entered into will be fruitful or not. My object in the present note is to contribute towards the elucidation of this question.

The statistics furnished in the Decennial Census Reports and in the Annual Reports of the Registrar-General, afford the means of calculating the marriage rate among the general population, but these are of no assistance to us in the present inquiry. First, as regards the rate of marriage, it would be clearly unsafe to assume that the rate deduced from statistics as to the general population, of which the working classes form a very large majority, would be applicable to the landed gentry. Secondly, the above mentioned statistics give us no information whatever as to the fruitfulness of the marriages entered into. It is therefore necessary to seek statistics in some other quarter, and statistics very trustworthy and perfectly suitable for our purpose, are found among the records of the British Peerage as contained in the various Peerages published annually. The facts given in them are not so numerous as could be desired for special purposes, but it is of even more importance that our statistics should be accurate than that they should be very numerous. It therefore seems to me that the statistics obtained from a careful examination of the records of the British Peerage may safely be adopted as the basis of our calculations in the present subject. I have accordingly taken the volume of Lodge's Peerage for the year 1871, and gone carefully through it, noting all the cases of marriages of men, whether bachelors or widowers, entered into after the age of 40. The total number of such men, as to whom the necessary information was complete, was 339, of whom 132 were bachelors and 207 widowers. These may seem small numbers on which to base a general law; but, in default of larger numbers, I think we must do the best we can to see what conclusions may safely be drawn from them. The following table shows the ages at which the marriages were contracted, and how many of those contracted at each age were fruitful or unfruitful. Most of the marriages included in the observations were contracted many years ago, so that the information contained in the volume of Lodge was taken as conclusive as to whether the marriages were fruitful or not. For the comparatively few as to which there seemed a doubt, I referred to the volume of Burke's Peerage, published in 1878. In all cases if a child was

born, I have deemed the marriage fruitful, whether the child survived or not.

Age at which the Marriage is contracted.	Bachelors.			Widowers.			Bachelors and Widowers.		
	No. of Marriages.	Of which were		No. of Marriages.	Of which were		No. of Marriages	Of which were	
		Fruitful.	Unfruitful.		Fruitful.	Unfruitful.		Fruitful.	Unfruitful.
40	25	19	6	11	8	3	36	27	9
41	15	12	3	7	4	3	22	16	6
42	19	14	5	10	7	3	29	21	8
43	10	7	3	14	12	2	24	19	5
44	6	3	3	10	5	5	16	8	8
45	6	4	2	10	7	3	16	11	5
46	8	5	3	4	3	1	12	8	4
47	8	6	2	8	5	3	16	11	5
48	6	6	...	11	10	1	17	16	1
49	6	3	3	2	...	2	8	3	5
50	2	2	...	7	4	3	9	6	3
51	2	...	2	7	5	2	9	5	4
52	2	1	1	10	10	...	12	11	1
53	3	1	2	8	5	3	11	6	5
54	4	3	1	4	2	2	8	5	3
55	2	...	2	7	3	4	9	3	6
56	...	...	...	9	3	6	9	3	6
57	2	1	1	7	4	3	9	5	4
58	1	...	1	10	5	5	11	5	6
59	2	1	1	3	...	3	5	1	4
60	2	...	2	14	5	9	16	5	11
61	...	...	...	1	1	...	1	1	...
62	...	...	...	4	1	3	4	1	3
63	1	1	...	4	...	4	5	1	4
64	...	...	...	1	...	1	1	...	1
65	...	...	...	7	2	5	7	2	5
66	...	...	...	3	...	3	3	...	3
67	...	...	...	2	1	1	2	1	1
68	...	...	...	...	...	...	...	...	...
69	...	...	...	1	...	1	1	...	1
70	...	...	...	2	...	2	2	...	2
71	...	...	...	2	1	1	2	1	1
72	...	...	...	2	...	2	2	...	2
73	...	...	..	1	...	1	1	...	1
74	...	...	...	1	...	1	1	...	1
75	...	...	...	...	...	...	...	..	...
76	...	...	...	1	...	1	1	...	1
77	...	...	...	...	...	...	...	...	...
78	...	...	...	1	...	1	1	...	1
79	...	...	...	1	...	1	1	...	1
Total	132	89	43	207	113	94	339	202	137

A glance at this table is sufficient to show that the probability

of a marriage being fruitful is less as the age of the husband increases, and the same conclusion appears more plainly when we group the ages quinquennially, as is done in the following table.

Ages at which the Marriages are contracted.	Bachelors.			Widowers.			Bachelors and Widowers.		
	No. of Marriages.	Of which were		No. of Marriages.	Of which were		No. of Marriages.	Of which were	
		Fruitful.	Unfruitful.		Fruitful.	Unfruitful.		Fruitful.	Unfruitful.
40–44	75	55	20	52	36	16	127	91	36
45–49	34	24	10	35	25	10	69	49	20
50–54	13	7	6	36	26	10	49	33	16
55–59	7	2	5	36	15	21	43	17	26
60–64	3	1	2	24	7	17	27	8	19
65–69	...	...	...	13	3	10	13	3	10
70–74	...	...	...	8	1	7	8	1	7
75–79	...	...	...	3	...	3	3	...	3
Total	132	89	43	207	113	94	339	202	137

If we calculate now the percentage of the marriages which are unfruitful in each quinquennium, we get the following results :—

Ages.	Bachelors.	Widowers.	Bachelors and Widowers.
40–44	26·67	30·77	28·50
45–49	29·41	28·57	28·99
50–54	46·15	27·77	32·65
55–59	71·43	58·33	60·47
60–64	66·67	70·83	70·37
65–69	...	76·92	76·92
70–74	...	87·50	87·50
75–79	...	100·00	100·00
Total	32·57	45·41	41·41

Here we see that, on the whole, both among bachelors and widowers, the probability of a marriage being unfruitful increases with the age of the husband. The progression among the bachelors is less regular than among the widowers; but this is no doubt to be explained by the smaller numbers that are under observation. When we take the bachelors and widowers together, the progression

is remarkably regular; in fact so regular, that, notwithstanding the comparatively small numbers observed, we shall be justified in believing that we have here an indication of a law of nature, upon which we may safely base our calculations.

In order to make our conclusions practically serviceable, it is necessary to calculate from them the probability for every age from 40 onwards, or, in technical language, to graduate the probabilities. This I do by a graphic method. In the appended figure I take the abscissa to represent the age, and the ordinate the probability that a marriage entered into at that age will be unfruitful. Thus, for age 42, being the middle of the five ages 40–44, the probability is ·285, and this is represented by the point A. In the same way the points B, C, D, E, F, G, H, represent the probabilities for the ages 47, 52, 57, 62, 67, 72, 77. Then, joining these points by straight lines, I draw a curve which shall follow the general course of the points as faithfully as is consistent with the avoidance of irregularities in its form. Then, by estimating the ordinates of the points where the curve cuts the vertical lines in the diagram, I obtain a first approximation to the adjusted probability for each age, and this is afterwards corrected by a process which it seems unnecessary to describe on the present occasion. The following are the values which I thus obtained :—

Probability that a Marriage entered into by a Man of any Age from 40 *onwards will be Unfruitful.*

A ge.	Probability.	Age.	Probability.	Age.	Probability.	Age.	Probability.
40	·284	51	·295	62	·702	73	·882
41	·284	52	·315	63	·720	74	·896
42	·285	53	·365	64	·738	75	·910
43	·285	54	·430	65	·755	76	·924
44	·286	55	·500	66	·772	77	·937
45	·286	56	·562	67	·789	78	·950
46	·287	57	·600	68	·805	79	·963
47	·287	58	·626	69	·821	80	·976
48	·288	59	·646	70	·837	81	·988
49	·288	60	·665	71	·852	82	1·000
50	·290	61	·684	72	·867		

If we now multiply the number of marriages entered into at any age, by the probability in this table, the product will be the number

of those marriages that may be expected to be unfruitful. Doing this for each age, and adding the products together in quinquennial groups, we get the following comparison of the expected and actual number of unfruitful marriages, the agreement between the results affording a very complete test of the goodness of the adjustment.

Ages.	Number of Unfruitful Marriages.	
	Actual.	Expected.
40–44	36	36·2
45–49	20	19·8
50–54	16	16·5
55–59	26	25·1
60–64	19	18·5
65–69	10	10·0
70–74	7	6·9
75–79	3	2·8
Total	137	135·8

I will not attempt to discuss the various points on which the fruitfulness of a marriage depends. The most important of them is clearly the age of the wife; and consistently with this, we see that the probability of a marriage being unfruitful, increases with great rapidity between the ages of 50 and 60; obviously because a much larger proportion of the wives married by men of these ages are past child-bearing than is the case with those married by men under 50.

It is but very rarely that the information available to the public will enable us to form even a reasonable conjecture whether the absence of issue of a marriage is attributable to the husband or to the wife, but such cases do occasionally occur. For instance, when a man marries a widow of 30 who has already had several children, it may fairly be inferred that it is the fault of the husband if there are no children born of her second marriage. So, again, if there are no children born of a marriage, the wife dies, and the husband then marries a second time and has a family, it may fairly be inferred that the absence of children of the first marriage was not attributable to him. It is obvious that cases of this kind are so very few that we cannot attempt to base any general reasoning upon them, nor is it all necessary that we should do so.

It only remains to mention that in obtaining the probabilities above set forth no account has been taken of the age of the wife at the date of marriage. Our conclusion therefrom will have no application to individual cases where the age of the wife is known, but are only applicable to the cases indicated at the outset, where the men we are considering are not contemplating marriage. In other words, we have calculated the probability that, if a man who is either now married or who is single and is not contemplating marriage, shall hereafter enter into a marriage at a certain age, the marriage will be unfruitful.

The following Gentleman was duly elected a Fellow of the Society:—

John Turnbull, Esq., of Abbey St Bathan's.

Monday, 19th May 1879.

Principal Sir ALEXANDER GRANT, Bart., Vice-President, in the Chair.

1. Notice of the Death of the President of the Society. By the Chairman.

Sir Alexander Grant said:—

Gentlemen,—We cannot pass to the proceedings of this evening without some reference to the calamity which has befallen the Royal Society of Edinburgh, and under the sorrowful impression of which we now meet—the sudden death of our honoured and well-beloved President.

We knew, alas! gentlemen, that his health had been failing of late, and that when, only six months ago, he first took his seat as President of this Society, his vigour was impaired, at all events for the time.

But when the lamp of his spirit blazed up so brightly, in the address which he delivered to the University, less than one month since, and which was received with pleasure and enthusiasm by the students and his colleagues, and all the large audience

that were gathered round—on that occasion, I say, who could have expected that his end was so near? He seemed to exhibit vital force such as might carry him through many a year more upon this earth.

Perhaps, had he listened to the first premonitions of disease, had he recognised the necessity of repose and inaction, this might have been the case. But he was of too ardent a nature to "husband out life's taper to the close," and amidst the regrets and lamentations which have now been called forth, may we not say that there was some consolation in that last public scene? May we not almost say that he was *felix opportunitate mortis?* He died, like a victorious warrior, with the affectionate cheers of the University which he had loved so well still ringing in his ears. He sank surrounded by the hues of a refulgent and happy sunset after a long bright summer day.

I shall only venture, gentlemen, to offer a few words of personal recollection of our friend whom we have lost.

His kindly presence seems to belong to this room, as it does also to the University, and to the very streets of Edinburgh. His sympathetic nature led him always to identify himself with the human interests among which he found himself thrown. As professor of mathematics for forty-one years, he was not only one of the best and most highly appreciated teachers that the University of Edinburgh ever had, but also one of its most loyal members and devoted champions. In University matters he had that true insight which is begotten by sympathy; so that though he was an Englishman, born and bred in an English parsonage, and educated at Cambridge till he was thirty years old, he is acknowledged to have understood the Scotch Universities—better almost than any one else. In his addresses and his conversation he loved to dwell more on the merits than on the imperfections of the Universities of this country, and he earnestly deprecated any reforms which should destroy the essential character of those Universities. He had, in the best sense, a thoroughly academic mind. Indeed, he was the type and model of an academic figure. Of genuine piety; with deep learning in his own subject; with a modest, seemly, and dignified exterior; he was full of bright pleasantry and the sweet amenities of life. His interests were not confined to the Universities; he

adopted and took to his heart the broad land of Scotland; and it was a labour of love with him to assist in administering one of the most important of the educational Trusts of this country. It would be utterly out of place—both on this occasion and for myself—to attempt to speak of the scientific merits which rendered Professor Kelland worthy, by universal consent, to hold the high place of President in this Society. I will now merely recall one or two of his own utterances, still full of meaning for us, extracted from those addresses, which were always so pleasing and always so characteristic of him. Whenever I have listened to or read these addresses, they reminded me of that description of a Roman worthy:

> Venit et Crispi jucunda senectus
> Cujus erant mores, qualis facundia, mite
> Ingenium.

> The pleasant old man, Crispus,
> Whose life and mind were, like his oratory,
> All in a gentle strain.

But there was more than mere gentleness in Professor Kelland's utterances. He had the art of conveying many deep and pregnant truths in apparently light and mirthful sentences. I never knew a lecturer who was at the same time so sunny and so wise. Had his life been prolonged and his health restored, we might have expected him often to delight us from this chair. But it has otherwise seemed good to Providence.

Nineteen years ago Professor Kelland returned to his class after being face to face with death in a terrible railway accident. He had travelled sooner than some thought prudent after the injuries he had received. He said, in an address to his students, "I believed that the path of duty is the safest and the easiest path, and I acted on this conviction when, against the advice of my friends, I came down suddenly amongst you." He spoke then of the deaths of no less than twenty-nine professors which had occurred since he had joined the University, and he added, "but I am spared a little while." In that address he seemed to rise to a survey of human life, especially a life spent in pursuit of science. He paid a noble tribute to the earnest genius of Professor George Wilson, then recently dead, whom he compared and contrasted in a most interesting way with the great mathematician Baron Canchy, and he quoted that beautiful saying, which might almost be considered to

have been the motto of Professor Kelland's life: "The measure of the happiness of a man is the number of things which he loves and blesses, which he is loved and blessed by." From the address which Professor Kelland delivered to us at the opening of this session, I must beg to quote three sentences. The first indicates the spirit in which he accepted the office of President. He said: "To myself this honour has come neither to gratify ambition nor to administer to self-conceit. It has descended on me all unsought, through the kindness of the many friends who have sat with me in this room, and the only emotion it awakens is that of affection and gratitude." In the second sentence which I shall recall, Professor Kelland evinces his warm interest in the rising generation of scientific workers. He says:—"One word which I venture on as both encouraging for the present and hopeful for the future, is the remarkable number of young men who are just entering on their work. In the fasciculus of the Society's Proceedings just issued, I count no less than eleven names of young men just entering on their career of investigation. How many of them have caught their inspiration from contact with those older workers who have been long among us? How many have been drawn out and cheered by the associations of this room." In the last sentence which I shall quote Professor Kelland teaches us how now we ought to regard himself. He says: "The feelings which arise on casting one's thoughts back through twenty years are full of sadness when they fasten on individual members of the Society whose presence at our meetings was a source of pleasure not unmixed with pride, but of sadness, brightened by glimpses of the future, when we think of them as members of a living body, as workers even now in the field which man has been sent into the world to cultivate—the field where truth is to be sought and found." As a member of that living body, as an immortal worker in that field of truth, with "sadness brightened by glimpses of the future," we must now think of Philip Kelland.

It was moved by the Chairman, "That the Society should request the Council to express to Mrs Kelland and family, the great regret experienced by the Society at the death of the President." The motion was carried unanimously.

The following Communications were read:—

2. Why the Barometer does not always indicate the Real Weight of the Mass of Atmosphere aloft. A continuation of the Paper on this subject laid before the Society in Session 1876 and 1877. By Robert Tennent.

Meteorological phenomena are in all cases to be regarded as being both a cause and an effect; owing to this, and also to the imperfect state of our knowledge on this subject, it may be safely asserted that exception proves or tests the rule. Taking up the subject in this point of view, much assistance is in this way to be acquired when attempts are made to explain the phenomena which are under consideration, and hence these negative conclusions can to a large extent be taken advantage of. As an illustration of this, can it be assumed that a barometer placed on the surface of the earth, and which always correctly indicates the amount of pressure on its cistern, will also always correctly indicate the weight of the mass of air aloft, both when it is at rest and when it is in rapid motion, accompanied by the important element of friction. Equal identity of pressure in both such cases is assumed, although no observations to justify the correctness of this assertion have been made. With the atmosphere in a state of perfect rest, setting aside just now the effects of heat and vapour, there will be an upward *normal* diminution of pressure, but with the same superincumbent mass of air no longer at rest, but attended with the dynamical element of motion in the rapid upper strata, will there not then be an upward *abnormal* diminution of pressure, due to the "lifting" of the air on the surface and its accumulation aloft? It is this point which was shown in the former paper, and is now here to be more fully explained. In both such cases, and under the same superincumbent mass of air, can the barometer, placed on the *surface*, possibly show the same amount of pressure? It is only by a series of barometers placed vertically above each other to a great height, and not very far apart, that the amount of pressure in both such cases will be found to be the same.

Mathematical meteorological investigations cannot be here introduced with perfect accuracy, and only to a small extent, where so much complexity, irregularity, and want of uniformity is to be

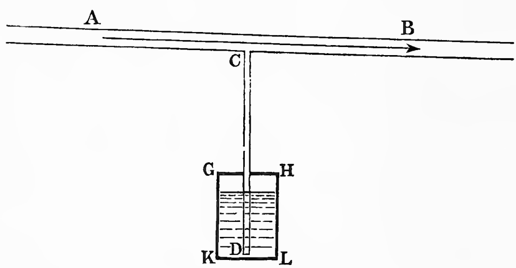
found, and where barometric observations are not always absolutely reliable, nor also are the gradients and isobars which depend upon them. In such an imperfect state of matters, exceptional cases must always be found, and these, if not disadvantageously too numerous, must consequently, as above remarked, be regarded as being a negative test or proof of the conclusions arrived at. The importance of barometric observations is however well known, but, as has been pointed out by the Astronomer Royal and others, a great mass of these is valueless and simply overwhelming, while comparatively few definite conclusions have as yet been arrived at. Hence explanatory theories must be produced to point out the particular direction in which such observations ought to be carried out. In such a subject as this, the greater the extent to which investigation takes place the greater will be the amount of difficulty and complexity, but such a discovery is only to be regarded as being a more accurate approach to ultimate reliable conclusions.

The object of this paper is to show, mainly in a mechanical point of view, why the fall of the barometer to a great extent is due to strong winds, setting aside at present the much smaller amount of fall produced by heat and vapour. In other words the point to be explained is,—why horizontal movement takes off vertical pressure. This fall, due to strong winds, is produced by a real *removal* of air and the amount to which it takes place will depend on the extent of the resisting surface over which the winds pass, and also on the amount of their source of supply, which is indicated so far by the steepness of the gradients. In a somewhat similar way, when the incline at the bottom of a river is very slight, the current will then move slowly, and will tend to deepen and accumulate, but when the incline is much steepened, the current will then increase in speed, removal will now take place, and it will consequently shallow out. Although the barometer thus falls for "removal" of air, it is intended to show that this fall is not entirely due to removal, but that it is also partly due to "lifting," which is an exhibition of fictitious pressure. The barometer has been described as being both a cause and an effect. An area of low pressure is a *cause* of the indraught of aerial currents, but owing to the peculiar mode in which they inflow to this low centre by rapid upper currents, a still further diminution of pressure will take place, as just above mentioned, and this

is to be regarded as being an *effect.* In another point of view, it may be here mentioned, that if a very swift wind blows over a convex surface, pressure will then be diminished, but if the convexity is that of the earth's surface, and the velocity that of actual winds, the effect will be far too small to be sensible.

These rapid upper currents and their effects, to which so much importance has been attached, do not consist merely of light rarefied upper aerial strata which can be possessed of little momentum. The mode in which they are constructed may be thus described. Their velocity begins to increase at only a few feet above the surface, as has been shown by observations; this increase goes on often to a great height in the atmosphere, much of it, however, is to be found in the more weighty and condensed mass of the air not much above the surface, where it must be accompanied by a powerful force due to its momentum. The weight of a cubic mile of air amounts to several millions of tons; where many such are in motion, their weight and their accompanying momentum will be sufficiently prodigious to produce the effects which have been pointed out.

From experiments made by Halley and Hawksbee, they came to the conclusion that horizontal movement took off vertical pressure, while Professor Leslie experimented to refute this.* A well-known experiment will illustrate this. If a current of water flows down an incline in the direction of the arrow AB, instead of falling down

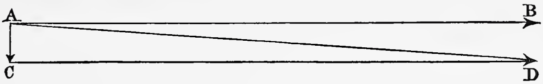


Diag. No. 1.

through the orifice at C, it passes over it and draws up or lifts through the small tube CD the water contained in the vessel GHKL. This is carried out at the expense of the momentum of the greater current, in which, in this way, an additional amount of water is *accumulated.* The gravity of the current as

* See "Trans. Roy. Soc. Edin." vol. xx. p. 377.

it passes over the orifice at C is undiminished; notwithstanding this, it does not here descend. Why it does not do so may be thus explained. Its gravity, which operates in a downward vertical direction AC, is here altered by its horizontal velocity,

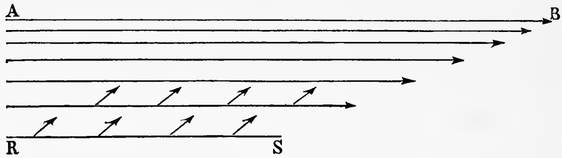


Diag. No. 2.

AB in the direction of the component AD. Attraction to the earth's centre remains undiminished; but if in this way it is distributed over a greater extent of surface, on any point of it, it may then practically be regarded as being really diminished. The weight of the current passing over the orifice at C may therefore be regarded as operating in an altered direction. With an incalculable amount of velocity, gravity may be said totally to disappear. The distribution of pressure may be exemplified by a cannon ball which, if it falls directly downwards from the mouth of the gun, will then show its full weight when it comes in contact with the surface of the ground, but if it moves rapidly in a horizontal direction its weight may now be regarded as being distributed over an extent of surface, on any point of which it is there diminished practically. When skating slowly over thin ice, it gives way, but by passing rapidly over it, this may be done in safety. The explanation of this is, that the ice has not here time to give way; this is no doubt so far correct; but it must also be observed, that the weight of the skater, if at rest, is to be found in a vertical direction and is then undiminished, but when he moves it is then distributed over the surface of the ice and is in this way lessened. The greater the amount of velocity, the greater will be the practical diminution of pressure over a corresponding extent of surface.

Let a current of air move rapidly along the incline AB in the mode above shown. Let the vessel GHKL, which held water, now only contain air, air will in the same way be drawn up, *lifted*, and *accumulated aloft*, in the rapid motive current AB. If the vessel is enclosed on the top, the tube by which the air is drawn up from it will produce within it rarefaction and diminution of pressure, and only a slight accumulation aloft, as its source of supply is now entirely restricted. Upward abnormal diminution of pressure in

the atmosphere caused by the introduction of the element of dynamical motion is due to this accumulation of the air aloft, and is consequently accompanied by increase of pressure there, and by diminution of pressure on the surface. Let the arrow AB represent



Diag. No. 3.

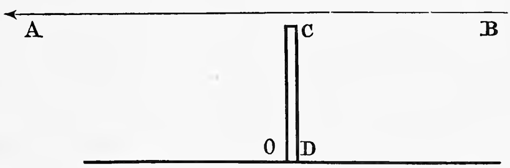
the strong upper current, and let the diminution in length of the arrows beneath it represent the diminution in the rate of their speed. As was shown in the diagram in the last paper* on this subject, these strong upper currents obtained much of their necessary source of supply by "lifting" it from the slower and more retarded surface currents beneath them. The direction in which this lifting takes place is shown by the small arrows inclined in the direction in which movement takes place. Accumulation is here indicated by the comparative closeness in the parallel position of the arrows, while the rarefaction and diminution of pressure on the surface, due to lifting, is indicated by their comparative wideness in the parallel position of the arrows there. It is only with a highly elastic fluid such as air, aided also by a certain amount of viscosity, and accompanied by horizontal surface retardation, that this alteration in the position of vertical pressure can take place. If the moving currents are non-elastic fluids, such as mercury, their rapidly moving upper strata will only cause a real diminution of pressure on the surface, but no increase of pressure or of accumulation aloft, because lifting cannot here take place. With a perfect fluid, also, no surface retardation will be found, and no difference in the speed of currents in so far as they are due to this cause. Hence here, as on a frictionless surface, "lifting" will not take place.

When a very rapid rise of the barometer takes place, as is frequently to be found in West Indian hurricanes, it may at least so far be accounted for in this way; when abnormal upward diminution of pressure is found due to accumulation aloft, as shown in the diagram No. 3, and when the motive force which produces it and

* Proc. Roy. Soc. Edin. 1876–77, p. 412.

holds it up in this position has come to a termination, a vertical rapid descent of the accumulation will of course take place to overcome the diminution of pressure on the surface, and in this way it will at least so far approach the form of upward normal diminution of pressure. A rapid fall of the barometer on the surface will also in a similar but in a reverse way take place, when accumulation begins to take place, with diminution of pressure on the surface. Upward *normal* diminution of pressure, which is here made use of as an exemplification of what takes place, is in all probability seldom or never to be found, as the atmosphere is rarely, if ever, in a state of perfect rest, nor is there any uniformity in the effects of heat and vapour. It is only in a mechanical point of view that it can assist in explanations.

When the wind, represented by the arrow AB, blows over the top of a wall CD, it will lift the air and diminish pressure on the lee side at



Diag. No. 4.

the point O, though only to a small amount, while the weight of the atmosphere may be said to be unaltered. This diminution of pressure is caused by the wall, which retards the velocity of the wind. Friction on the surface of the earth may be exemplified by a long series of such walls, which will produce diminution of pressure immediately above them. It is only on a very extensive surface of, say several hundred miles in length, with a corresponding width, that strong upper currents can produce lifting and diminution of pressure to its full extent; under such circumstances, the comparative height of these upper currents will not be great as compared with their length. Redfield has pointed out that depressions have often a height of only $\frac{1}{200}$th part of their width, while in other instances their height may be comparatively great in reference to their width, in which case little or no diminution of pressure will be found. The effect of such an extensive resisting surface is to produce retardation of the surface currents, along with an imperfect amount

of supply. If these are not found, "lifting" will not take place. The importance of the difference in the comparative height and length of the currents, and of the depressions, in a mechanical point of view, was taken advantage of in the last paper* to show how depressions opened out in front, and by so doing moved forward.

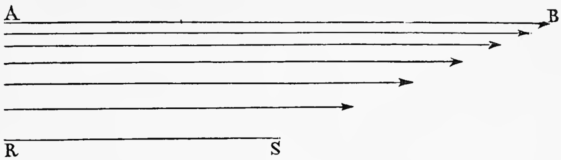
An exemplification of the effect of an extensive surface which produces low pressure may be found in the southern hemisphere below lat. 40°, where what are called the " roaring forties " are to be found, in often interminable gales from a westerly direction, and with little intermission, and extend over a vast portion of the globe. The Rev. S. J. Perry, in his voyage there, found the average height of the barometer there to be in November, 29·658° ; in December, 29·462°; in January, 29.406° ; February, 29.610°. This may therefore be accounted for by removal and lifting. In other countries, where calms and light winds prevail,—where strong winds blow only for a short time over a small extent of surface, as in the Mediterranean, no such low pressure is to be found. In explanations attached to the barometer, to show the effects of its rise and fall, calms are predicted when the mercury is high, as a general rule. Local exemplifications of lifting from the bottom of a valley are to be found in the Fohn in Switzerland, where the wind passes rapidly over their summits, and are also to be found on the lee side of precipices.

In the diagram No. 1, where the current of air AB lifts up the air from the vessel GHKL, which is now enclosed on the top, and has now its source of supply arrested, it is then that rarefaction and diminution of pressure takes place to the full extent. This may also illustrate the effect of an extensive resisting surface in arresting the source of supply to its surface currents and causing their retardation. In a former paper† it was stated that, in weather charts, the constant rise and fall of the barometer, which is there reported, *is to a large extent simply due to the passage of air over a resisting surface ; on a frictionless surface these mechanical effects would be entirely removed.*

There is an important difference in the mode in which Equatorial as compared with Polar winds, inflow to a low centre. Let these

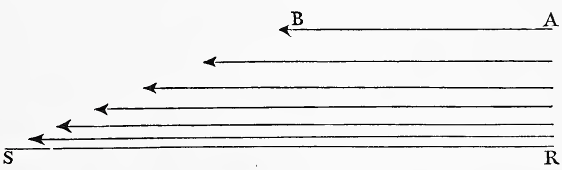
* Proc. Roy. Soc. Edin. 1877–78, p. 572.
† Proc. Roy. Soc. Edin. 1876–77, p. 414.

be exemplified by S.W. winds, which cause a fall in the barometer, while N.E. winds create a rise. South-west are accompanied by rapid upper currents, and let these be represented in the diagram No. 5 by the arrow AB, while the less rapid currents beneath are exhibited by the shorter arrows. RS is the surface of the ground.



Diag. No. 5.

Let the narrowing in the parallel position of the upper arrows represent accumulation aloft, and their widening below represent rarefaction and diminution of pressure. In this way, and also owing to removal of air, the fall of the barometer with these winds may be shown. North-east winds are not accompanied by rapid upper currents, which move somewhere about the same rate of speed as those on the surface. For the sake of illustration, let it be supposed that they move even more slowly, and let them be indicated by the shorter arrows AB above, while the more rapid surface currents passing over the surface RS are shown by the longer arrows beneath. In this case



Diag. No. 6.

surface retardation, in so far as it is indicated by the difference betwixt the comparative speed of the currents below and above, will not here be found, although, of course, it really takes place. Accumulation aloft cannot here be found; it will now take place on the surface, as is shown in this diagram by the closeness of the parallel position of the arrows there. This tendency to condensation, which was also to be found in the former instance, was there removed by the rapid upper currents, which here are not to be found. With these N.E. winds, therefore, as removing and lifting does not take

place, they are accompanied by a rise of the barometer. The mode in which these N.E. surface winds are enabled to overcome the effect of friction, and to accumulate on the surface, may be thus explained. They are cold and dense, and they enter underneath a milder atmosphere in the form of a wedge and raise it above them. To enable them to do this they must have a copious source of supply from an area of high pressure, and they are also aided generally by descending currents, which possess a great amount of potential energy.

As is well-known by seamen, these surface N.E. winds appear with a remarkable amount of suddenness and violence, and the disasters which accompany them have been often recorded. Southwest gales which blow aloft approach much more slowly, and, as this approach is first exhibited aloft, they can be predicted with much certainty.

In the last paper just alluded to it was mentioned that the range of the thermometer is equally great both above and below its mean. But with the barometer, the extent of its range above the mean is not more than one-half of that which takes place when it is below it. When it is below the mean, Equatorial winds generally prevail, such as those S.W. just alluded to, which are accompanied by removal and lifting. When above the mean, Polar winds and calms prevail, which are not accompanied by removal and lifting. Hence, as a general rule, Equatorial winds exhibit an amount of fictitious pressure, while Polar winds show more nearly, real or statical pressure, which, however, may possibly be so much in excess. Much important information as to the mobility of the upper aerial strata may be shown by the experiments made by Professor Tyndall at Chamouni, and at the summit of Mount Blanc.

From observations made by the late Mr Johnson, director of the Oxford University Observatory, it was found that, at a height of 110 feet from the ground, the velocity of the wind was found to be $2\frac{1}{4}$ times that indicated by an instrument at 22 feet above the soil. This velocity, of course, increases with the elevation, and to the extent of five or six fold, as shown by Glaisher. In the address of the President, H. S. Eaton, on 21st February 1877, he alludes to the ascent of a balloon from Paris. At an elevation of 6560 feet, a violent wind was encountered, and on

reaching a point 187 miles north of Christiania, and nearly 1000 miles from Paris in a direct line, the balloon must have sailed at the rate of at least 66 miles an hour, while nowhere did the extreme speed of the wind registered at any observatory at all approach the speed of the balloon. When the stratum of clouds overhead is of sufficient depth, and when the difference in the velocity of their different strata is sufficiently great, a columnar inclination of the clouds in the direction in which they move will be observed. This will illustrate the inclination of the columns of air which represent the increasing upward velocity of the winds, and is so often referred to.

Over the ocean, as seamen are well aware, sea birds, when strong winds prevail, attempt to fly only on the surface, where their velocity is retarded, while above this they are unable to move against them.

To ascertain the amount of diminished or fictitious pressure which is due to lifting, the real weight of the same mass of atmosphere must be ascertained both when it is at rest and when it is in motion. The results of such an observation, if made by a series of barometers placed vertically above each other to a great height, and not very far apart, will, in each case, exhibit an equal amount of pressure, and when the atmosphere is at rest, the barometer on the surface will then alone show its true weight, but when it is in motion, the barometer on the surface will then indicate diminished or fictitious pressure, because the air is then so far lifted and rarefied immediately above the surface, and is accumulated aloft with increase of pressure there. The result of this, as shown, is an upward abnormal diminution of pressure. The barometer always shows the real pressure of the air in contact with its cistern, which it does in this latter instance, but it does not here show the weight of the superincumbent air, while in the former case it showed both accurately. In this way, and also for other reasons, it does not indicate correctly the height of mountains, nor can observations made at some height and then reduced to sea-level be at all dependable.

Observations with a series of barometers as thus just above suggested, will be best carried out in a wide open atmosphere over the sea, and not in contact with the slopes of a lofty mountain, which,

for many reasons, must necessarily derange the observations. As no such direct observations can be made at all, results can only be arrived at from those which are not absolutely definite and conclusive. Aided by theory, some of these may be here suggested.

Observations were made by Captain James in his cottage at Granton during the prevalence of strong winds.* When this took place in the form of violent gusts, the instrument placed in one of the rooms, where, of course, it was so far confined, showed then a considerable lowering of pressure, while that placed outside remained comparatively unaltered. Could this cottage have been so arranged or constructed as to move forward at a uniform rate of speed with the gusts, pressure inside would not then at all be reduced, because no local confinement or retardation of the air would then take place; its amount would then be the same as that shown by the instrument outside.

Experiments were made by the writer at Craigleith Quarry, near Edinburgh, with a barometer placed at the top and at the bottom: the depth of the quarry, as compared with its width, is considerable. The remarkable difference at these spots was this, that while the height of the mercury, as observed at the upper part of the quarry, remained uniform, or nearly so, during strong gusts; at the bottom, its oscillations were very great, with much occasional diminution of pressure, which then took place when the gusts passed rapidly over the upper surface, to which the air was drawn up from the bottom; "lifting" and accumulation aloft are thus exemplified. At the bottom, where the air was confined, retardation took place. Had there been no such confinement—had supply there been equally copious as on the surface, no such oscillations would have taken place. This is also illustrated by a waterspout, as shown in the first paper on this subject in 1875.

Observations to throw light upon the subject in question may be made in the following manner: When a strong wind blows over the sea, let there be placed at anchor in the direction in which it blows a line of vessels with instruments placed on deck to show pressure. The wind passing over them may then be represented as moving in inclined columns, in which case the barometer does not indicate the real mass of the air aloft. Let another vessel now move forward in

* See "Trans. Roy. Soc. Edin." vol. xx. p. 377.

the direction and at the same rate of speed as that of the wind, a calm will now prevail over its deck, and the columns above it may then be regarded as being vertical, and indicative of the real weight of the air aloft; when it passes close alongside of those at anchor, a difference of pressure will be found on each, although the mass of air aloft is the same. For various reasons, however, the real amount of difference here will not in this way be correctly ascertained, and it will not be great. If a calm prevails over the vessels at anchor they will then show the real weight aloft. The moving vessel will now produce and encounter a force of wind equal to her rate of speed, and will not now show real weight aloft.

Vessels crossing the Atlantic against the strong prevailing west winds, will increase their force in proportion to the rate at which they sail against them. They must, therefore, indicate a difference of pressure when passing those vessels returning with the winds in their favour. Its amount will not be great, because the extent of the resisting surface which is represented by their decks is insufficient to produce retardation of horizontal and lateral supply. If, however, the instruments are placed in some enclosure, as was seen in the cottage, some amount of difference of pressure will then be shown.

For want of space here, and owing to the extreme difficulty of the subject, it cannot here be fully explained, but it may be stated that the conclusion has been arrived at, that as a general rule, the barometer in the Tropics, with, of course, exceptional cases, more nearly shows the real weight of the atmosphere than it does in more northerly latitudes. In countries of very different descriptions of structure and climate, this will probably be found to be the case, and also over the land and the sea.

The isobars, which are constructed on the supposition that the barometer always shows the real weight of the air, cannot be accurate. In the different segments which surround an area of low pressure, they will require a difference in the amount of their correction.* In the same way, and for other reasons also, gradients which are figuratively equal, as is well known, do not exhibit the same amount of incline, or the same amount of inflow of the winds. Correction here is evidently also necessary.—*May* 1878.

* See "Proc. Roy. Soc. Edin." 1874–75, p. 618.

3. On Some New Bases of the Leucoline Series. Part II. By Mr G. Carr Robinson and Mr W. L. Goodwin.

4. On a Calculus of Relationship. By Alexander Macfarlane, M.A., D.Sc., F.R.S.E.

1. De Morgan read a paper on the "Logic of Relations" before the Cambridge Philosophical Society, and the paper is printed in their Transactions. He attempts to deal not only with the idea of *relationship*, but with the idea of *relation* in general. As he does not deal with exact ideas, he cannot give any exact results.

2. Among the writings of Leslie Ellis there are printed some notes on Boole's "Laws of Thought," and there he refers to the idea of relation. He makes the important remark:—"It seems to me that the mind passes from idea to idea in accordance with various principles of suggestion, and that, in correspondence with the different classes of such principles of suggestion, we ought to recognise different branches of the general theory of inference." But he proceeds to discuss equations expressing any kind of relation, thus neglecting his own principle of making a special investigation for each really different kind of idea.

3. What I have attempted to do is to devise a complete analytical notation for what can be represented graphically by means of a genealogical tree, to consider how data about relationships should be expressed, and to point out rules for manipulating these data.

4. The distinction between the *relationship* and the *persons between whom the relationship exists*, can be represented by means of small letters in contradistinction to large letters—*e.g.*,

$$s\mathrm{A} = \mathrm{B} + \mathrm{C} + \mathrm{D}$$

The sons of A are B, and C, and D.

5. The symbol s has a definite arithmetical value, which is an integer. It is a multiplier, or operating symbol, which changes A into B and C and D.

6. To express any relationship whatever, we require only four fundamental symbols expressing the four fundamental relationships (1) of a father to his sons, (2) of a father to his daughters, (3) of a

mother to her sons, and (4) of a mother to her daughters. These may be denoted respectively by

$$s\,, \quad d\,, \quad \sigma\,, \quad \delta\,.$$

7. *Superior indices* and *inferior indices.* In an investigation we may require to consider several different s relationships; these are best distinguished by means of superior indices, *e.g.*,

$$s^1, \quad s^2, \quad s^3.$$

We also require to consider one of the s's, say the nth; this is properly denoted by s_n.

8. The sign $=$.

Let

$$sA = B + C + D\,.$$

This equation asserts that the sons of A comprise B, C, and D exactly, neither more nor fewer.

The equation $$sA = \sigma B$$
denotes that the sons of the man A are identical with the sons of the woman B. The truth of the equation involves

$$\bar{s} = \bar{\sigma}\,,$$

where the bar denotes that the arithmetical value of the symbol is taken. What $=$ denotes is *identity* of persons—not necessarily identity of relationships. Equations which express the latter idea may be called identities.

9. The sign $+$ has its ordinary meaning. For example—

$$sA = \sigma^1 B + \sigma^2 C$$

The sons of the man A are identical with the sons of the woman B, together with the sons of the woman C.

10. The law $$s + d = d + s\,.$$
It is obvious that the formal law

$$(s + d)A = (d + s)A,$$

which means—

> The sons together with the daughters of A are identical with the daughters together with the sons of A—

is true whoever A be.

11. The sign $\times$.

$$s^1{}_m\ s^2{}_n\ A$$

denotes the mth son of the nth son of the man A. The operators

s^1 and s^2 are similar as regards their nature, but are different individually. Consequently their arithmetical values may be different.

12. In this branch of analysis, order is essential. The operator s^2 is prior to s^1. Hence the commutative law does not apply to symbols connected by $\times$.

$$s^1{}_m s^2{}_n \text{ is not } = s^2{}_n s^1{}_m .$$

In this respect the branch of analysis which investigates relationship stands in contrast to the branch of analysis which investigates quality. For in the latter

$$xy = yx .$$

At the same time it agrees with the Quaternionic analysis, for in it

$$pq = qp$$

is not true in general. (Tait's "Quaternions," p. 37.)

13. I may here quote a few sentences from a paper by the late Professor Clifford, which throw light on this subject—

"There are two sides to the notion of a product. When we say $2 \times 3 = 6$, we may regard the product 6 as a number derived from the numbers 2 and 3, by a process in which they play similar parts; or we may regard it as derived from the number 3 by the operation of doubling. In the former view, 2 and 3 are both numbers; in the latter view 3 is a number but 2 is an operator, and the two factors play very distinct parts." The product of two *quality* symbols is of the former kind; the product of two *relationship* symbols is of the latter kind.

14. $$s_m A = B$$

asserts that B is the *m*th son of the man A.

$$A = \frac{1}{s_m} B$$

asserts that A is the father of the man B, and that B is the *m*th son. Thus,

$$\frac{1}{s_m}$$

does not only denote "father of a male," but introduces the proper number to individualise the relationship.

15. Suppose that we have given the two equations

$$s_m A = B, \qquad \text{and} \qquad s_n A = C\,,$$

then
$$B = s_m \frac{1}{s_n} C\,,$$

and
$$C = s_n \frac{1}{s_m} B.$$

The expression $s_m \frac{1}{s_n}$ denotes the relationship of sons of the same father.

If $m = n$, then $B = C$.

Thus the general analytical expression for brother includes *oneself.*

16. De Morgan remarks on the difficulty commonly experienced by persons of putting together two (not to say more than two) conditions about relationship, that is, of deducing the conclusion from two given data which really afford a conclusion. He was accustomed to propound the following story, among others, as a test:—An abbess observed that an elderly nun was often visited by a young gentleman, and asked what relation he was. "A very near relation," answered the nun; "his mother was my mother's only child."

Let G denote the gentleman, and N the nun. Then the conditions given are—

$$\frac{1}{\sigma} G = \delta \frac{1}{\delta} N\,, \qquad \text{and } \bar{\delta} = 1\,.$$

Since $\bar{\delta} = 1$,
$$\delta \frac{1}{\delta} = 1 \quad (\text{Art } 15),$$

$$\therefore \qquad \frac{1}{\sigma} G = N$$

$$\text{or} \qquad G = \sigma N\,.$$

That is, the gentleman was the son of the nun.

When the visitor is not said to be young, the problem presents still greater difficulty to the ordinary intelligence.

17. Suppose
$$s_2 s_4 A = s_2 \frac{1}{s_1} \frac{1}{d_1} M\,,$$

that is, the second son of the fourth son of A is identical with the second son of the father of a man his first son who was the father of a woman his first daughter M.

Then $$\frac{1}{s_2}\, s_2\, s_4\, \mathrm{A} = \frac{1}{s_1}\, \frac{1}{d_1}\, \mathrm{M},$$

and $$\frac{1}{s_2}\, s_2 = 1 \qquad \text{(Art. 24)},$$

$$\therefore \quad s_4\, \mathrm{A} = \frac{1}{s_1}\, \frac{1}{d_1}\, \mathrm{M}. \qquad (1)$$

Also $$s_1\, s_4\, \mathrm{A} = \frac{1}{d_1}\, \mathrm{M}; \qquad (2)$$

$$d_1\, s_1\, s_4\, \mathrm{A} = \mathrm{M}; \qquad (3)$$

and $$\mathrm{A} = \frac{1}{s_4}\, \frac{1}{s_1}\, \frac{1}{d_1}\, \mathrm{M} \qquad (4)$$

Hence the rule for changing a relationship function from one side of an equation to the other is,

Invert each symbol of the function which is to be transposed, reverse the order of the symbols, and prefix before the function which is on the other side.

18. As an example of the value of the notation of this calculus, considered merely as a shorthand, I may express the relationship between Queen Victoria and Mary Queen of Scots—

$$\mathrm{V} = d\, s_4\, s_1\, s_1\, s_1\, \sigma_1\, \delta_a\, d_b\, \sigma\, \mathrm{M}.$$

Here there is no need in two cases to put a suffix, because $\bar{d} = 1$ and $\bar{\sigma} = 1$.

This relationship can be expressed in nine other forms, each equivalent to the above form, by taking over d, ds_4, ds_4s_1, &c., to the other side, after having transformed the expression in accordance with the Rule of Art. 17.

In two cases I have used a and b because I am uncertain of the actual numbers.

19. The signs $>$ and $<$.

$$\mathrm{B} + \mathrm{C} + \mathrm{D} = s\mathrm{A}$$

asserts that B, C, and D, and no others, were sons of A.

$$\mathrm{B} + \mathrm{C} < s\mathrm{A}$$

asserts that B and C, at least, were sons of A.

Thus the sign $<$ means *are included in*, as in the analysis of quality.

This is, of course, an important point. Leslie Ellis expresses

Shem was a son of Noah

by

$$S = sN\,;$$

but, so far as I have yet studied the matter, I am inclined to hold that s must be considered as in general a plural symbol, and that

$$S = sN$$

asserts implicitly that Shem was the only son of Noah. The truth

Shem was a son of Noah

is properly expressed by

$$S < sN\,;$$

but the truth

Shem was the eldest son of Noah,

by

$$S = s_1N\,.$$

20. Let f denote *father of* and m denote *mother of.* Then

$$f = \frac{1}{s+d} \quad \text{and} \quad m = \frac{1}{\sigma+\delta}\,.$$

21. *To find expressions for one's ancestors of the nth generation back.*

$$f + m$$

$$f + m\,.$$

Multiply together

$$\text{then } ff + mf + fm + mm\,.$$

These are the expressions for one's grandparents.

Multiply again by $f + m$,

then $fff + mff + fmf + mmf + ffm + mfm + fmm + mmm\,.$

These are the expressions for one's great-grandparents.

And so on.

The maximum number of ancestors of the nth generation back which a person can have is 2^n, and the minimum number according to the Laws of Consanguinity is 4.

22. According to the above, the relation of great-grandmother may denote any one of the four different relations—

$$mff\,, \qquad mmf\,, \qquad mfm\,, \qquad mmm\,;$$

and, taking into account the gender of the great-grandchild, there will be eight different relationships.

23. The notation I have framed is of great use in showing the ambiguities of the common terms of relationship. Thus uncle may may mean any one of eight things, or a combination of these. For example—

$$\begin{array}{ccc}
\text{Uncle and Nephew.} & \qquad & \text{Uncle and Niece.} \\
\left\{\begin{array}{l} s_1\frac{1}{s_4}\,\frac{1}{s_2} \\ \sigma_1\frac{1}{\sigma_4}\,\frac{1}{s_2} \end{array}\right. & & \left\{\begin{array}{l} s_1\frac{1}{s_4}\,\frac{1}{d_2} \\ \sigma_1\frac{1}{\sigma_4}\,\frac{1}{d_2} \end{array}\right. \\
\left\{\begin{array}{l} s_1\frac{1}{d_4}\,\frac{1}{\sigma_2} \\ \sigma_1\frac{1}{\delta_4}\,\frac{1}{\sigma_2} \end{array}\right. & & \left\{\begin{array}{l} s_1\frac{1}{d_4}\,\frac{1}{\delta_2} \\ \sigma_1\frac{1}{\delta_4}\,\frac{1}{\delta_2} \end{array}\right.
\end{array}$$

The relationships bracketed together generally coexist.

24. To prove that

$$\frac{1}{s}s \text{ always } = 1\,.$$

Let
$$A = \frac{1}{s}sB\,,$$
then
$$sA = sB\,.$$

But that is morally, if not physiologically, impossible unless $A=B$,

$$\therefore \qquad \frac{1}{s}s = 1\,.$$

Similarly
$$\frac{1}{\sigma}\sigma = 1\,.$$

Observation.—Neither $\frac{1}{s}\sigma$ nor $\frac{1}{\sigma}s$ can be equal to 1.

25. To express that

A is the brother of the brother of B.

The expressions for brother are—

$$\text{half-brother } s\frac{1}{s}\,, \qquad s\frac{1}{d}\,, \qquad \sigma\frac{1}{\sigma}\,, \qquad \sigma\frac{1}{\delta}\,;$$

and full brother
$$\left\{\begin{array}{l} s\,\frac{1}{s} \\ \sigma\,\frac{1}{\sigma} \end{array}\right\}, \qquad \left\{\begin{array}{l} s\,\frac{1}{d} \\ \sigma\,\frac{1}{\delta} \end{array}\right\}.$$

Hence brother of brother is denoted by

$$(1) \qquad s\frac{1}{s}\;s\frac{1}{s} = s\frac{1}{s} \qquad \text{(Art. 24)}$$

$$(2) \qquad s\frac{1}{s}\;s\frac{1}{d} = s\frac{1}{d} \qquad \text{,,}$$

(3) $s\frac{1}{s}\ \sigma\frac{1}{\sigma}$

(4) $s\frac{1}{s}\ \sigma\frac{1}{\delta}$

(5) $\sigma\frac{1}{\sigma}\ s\frac{1}{s}$

(6) $\sigma\frac{1}{\sigma}\ s\frac{1}{d}$

(7) $\sigma\frac{1}{\sigma}\ \sigma\frac{1}{\sigma} = \sigma\frac{1}{\sigma}$ (Art. 24)

(8) $\sigma\frac{1}{\sigma}\ \sigma\frac{1}{\delta} = \sigma\frac{1}{\delta}$,,

Subscript letters are to be understood. If in the case of 1 or 7 the subscript letters are the same, then

$$A = B\,.$$

26. To prove that $$s\frac{1}{\sigma} = 0\,.$$

Let $$A = s\frac{1}{\sigma}B\,;$$

that is, let A be the son of a male who is the mother of the male B.

But this is impossible in the case of the human species, where sex is monœcious. Hence A is imaginary; and therefore

$$0 = s\frac{1}{\sigma}B\,,$$

whoever B is.

27. The different permutations of the four fundamental symbols used directly and inversely may be exhibited in a table. I append one-fourth part of the complete table, marking the expressions which are impossible or which denote coincidence.

ss	$s\sigma$	$sd = 0$	$s\delta = 0$
$s\frac{1}{s}$	$s\frac{1}{\sigma} = 0$	$s\frac{1}{d}$	$s\frac{1}{\delta} = 0$
$\frac{1}{s}s = 1$	$\frac{1}{s}\sigma$	$\frac{1}{s}d = 0$	$\frac{1}{s}\delta = 0$
$\frac{1}{s}\frac{1}{s}$	$\frac{1}{s}\frac{1}{\sigma} = 0$	$\frac{1}{s}\frac{1}{d}$	$\frac{1}{s}\frac{1}{\delta} = 0$

28. The Marriage with a Deceased Wife's Sister Bill would make the following among other equations possible:—

$$s_a = \sigma^2{}_b d_c \frac{1}{d_d} \frac{1}{\sigma^1{}_e} s_f .$$

29. As the analysis of relationship is important not only in itself, but also as throwing light upon the nature of operators in Mathematics, I propose to continue the investigation, and to bring the results before the Society at a future meeting.

Monday, 2d June 1879.

Sir C. WYVILLE THOMSON, Vice-President, in the Chair.

The following Communications were read:—

1. On the Carboniferous Volcanic Rocks of the Basin of the Firth of Forth: their Structure in the Field and under the Microscope. Second Paper. By Professor Geikie.

2. Additional Observations on the Fungus Disease affecting Salmon and other Fish. By A. B. Stirling, Assistant Curator of the Anatomical Museum of the University of Edinburgh.

In my former paper, read before the Society in June 1878,* I gave an account of observations which I had made on the fungus disease affecting salmon, and described the character of the fungus, which I referred to *Saprolegnia ferax.*

In the present communication I propose to relate additional observations, and to discuss the theories which have been advanced by different writers in explanation of the cause of the disease. Four theories have been advocated, namely—pollution of rivers, overcrowding, absence of frost, diseased kelts and addled ova.

In reference to the theory that pollution is the cause of the

* See *Proceedings* of that date.

fungus disease of salmon and other fish in the rivers at present affected by it, I think it is only necessary to relate the fact, that diseased fish are found in those rivers many miles above all sources of pollution, to prove that it cannot have originated from that cause. In the Eden River, for twelve miles above Carlisle, a district in which there is no big town or other sources of pollution, the fungus disease has been found as deadly as below Carlisle after the sewage of the city has entered the river. In the Tweed also, both trout and greyling, which are non-migratory fish, have been found affected with fungus where no source of pollution is known to exist; for I have obtained trout from near Broughton, and greyling from near Stobo, both of which are from seven to ten miles above Peebles, the town highest up the Tweed. Mr Buckland also, in his Seventeenth Report on the Salmon Fisheries, England and Wales, 1878, states "that we must look to other circumstances in order to diagnose the origin of the mysterious disease."

The theory of overstocking as the cause of the disease has been advocated by Mr Buckland in the same report. He considers that "owing to the absence of freshes (spates) in a river, the spawned fish do not find their way to the sea, so that they accumulate in the pools in which the disease breaks out amongst them, as gaol-fever affected the crowded prisons in former times."

In May 1874 the Tweed Commissioners constructed a small pond for experimental purposes, which measured 36 feet by 16 feet, on the side of a small stream called Carham Burn, from which a run of water was supplied to the pond by a drain pipe. On 7th May 1874, 130 sea-trout smolts, the average length of each being 8 inches, were taken from the Tweed and placed in this pond. After an interval of two years they were specially examined, weighed, and measured on the 25th May 1876. Seventy fish were found in the pond, the average length of each was $12\frac{1}{2}$ inches; they were now in the whitling stage, and in fine condition. After another interval of two years there was another examination, when they were weighed and measured on the 23d May 1878, when sixty-six sea-trout, of the average length of $14\frac{3}{4}$ inches, were found in the pond.

In the interval between the examinations, and probably in the season 1876–77, the fish had spawned in the pond, and a numerous

progeny of parr and orange-fins were found along with them, all of the fish being in fine condition—the kelts being well mended. On 25th July of the same year they were, on the occasion of the pond being cleaned, again weighed and measured; they now averaged 15 inches each. They were again returned to the pond, and retained there till the 22d of May 1879, when thirty of them were marked with silver wires and returned to their native Tweed.

It will thus be seen that sixty-six large fish, whose united length is 80 feet, along with multitudes of parr and orange-fins, were cribbed, cabined, and confined for five years in a pond no larger than an ordinary dining-room, and remained in health during that period without exhibiting any signs of fungus disease, and this although the pond is situated within a few hundred yards of the Tweed—an affected river.

There can be no comparison, I submit, between a salmon-pool in a river—where the full current of the stream flowing through the pool provides for a constant change of water—with a confined pond fed only by a small pipe of water and crowded with fish; and yet, in the latter, no disease or death, other than that of kelts after spawning, has ever been detected.*

* Since this reference to the Carham pond fish was read to the Society, it has been stated by Inspector Johnston of the Berwickshire Police that several fish had been found dead in the pond, which, in his opinion, had died of fungus disease. Those deaths, of which there can be no doubt, took place between 11th February and 3d May 1879, embracing a period of eighty-two days, during which five fish had been found dead in the pond by Mrs Robson, the gamekeeper's wife, who fed them. These fish were shown to Inspector Johnston, who apparently paid official visits to the pond, and from the appearance of the fish he concluded they had died of fungus disease. I do not accept Mr Johnston's opinion on this point. He was well aware that I was engaged in a scientific investigation of the disease; indeed he had, by order of Mr List, chief-constable, caught in the Tweed and forwarded to me several salmon to aid me in my research. It is singular then, that, knowing the interest I took in the pond fish, he was silent, and did not at the time report upon the disease, which, according to his version, had existed in the pond for eighty-two days, a period of sufficient length for the fungus to have destroyed the fish, both root and branch; also, according to his own statement, no one saw the dead fish, with the exception of himself and Mrs Robson, and probably Mr Robson, the gamekeeper; and, consequently, there is no scientific evidence that the cause of death was *Saprolegnia ferax;* and, to quote the words used by Mr List in a letter to me of 21st June, "if *Saprolegnia ferax* had been in the pond, it must have been seen on the fish on the 22d May, when we saw every one of them." Taking those facts into consideration, I adhere to my statement that

The theory of absence of frost is also advocated by Mr Buckland, on the ground that the frost kills the spores of the fungus, and prevents them from germinating.

Now, if this were the case, there ought to have been no disease during the past winter, as the severe and long-continued frost should have killed the spores. But we know that the disease has been of a most virulent character in the Eden, Tweed, and other affected rivers. But, further, I may add certain definite facts which show that the disease may spread even where a river is coated with ice.

On 5th February I received from J. Dunne, Esq., chief-constable of Cumberland and Westmoreland, four salmon which were taken alive from the river Caldew, and, along with them, a report by Inspector John Nicholson, who observed them for a period of nineteen days. Annexed is a copy of Inspector Nicholson's report.

"CONSTABULARY STATION,
"EDEN TOWN, *4th February* 1879.

"SIR,—I beg most respectfully to inform you that on the 16th of last month five salmon were seen by me in a pool in the river Caldew, at Holme Head Bridge, one of which had a small white mark on the end of its nose, and which I thought showed symptoms

the case of the Carham pond fish fully proves that overcrowding is not the cause of fungus disease.

On the other side of the question—The pond-fish had been at least ten times specially examined during the five years they had been detained in it. By invitation of the chairman, I was present on two of those occasions, along with members of the experimental committee, Mr List the conductor of the experiment, a number of other gentlemen and practical fishermen, and it was a matter of surprise to all present that the fish were found in such fine condition.

At the final examination, which took place on 22d May 1879, I was prevented from being present, but arrangements were made that if any fish were found bearing marks of the disease they were to be transmitted to me. On the following day Mr List wrote to me that the "fish were in splendid condition for kelts, not the slightest sign of disease on any one of them."

It is well known among taxmen, practical fishermen, bailiffs, and anglers, that it is usual to find dead and dying salmon and sea-trout in rivers every season after they have spawned. This kind of mortality has been observed and written about for upwards of two hundred years. Isaac Walton mentions this as well known in his time, and there is no reason why the Carham détenus should be an exception to this rule, seeing they had spawned twice or thrice during their detention.

of fungoid disease. I removed other two salmon on the 19th of the same month from the mill-dam at Holme Head, and put them into the same pool for safety (they having been left nearly dry), making a total of seven salmon. On the following day I noticed a second marked on the dorsal fin. Since the last mentioned date I have not been able to see them distinctly, in consequence of the pool being frozen over with ice, until yesterday. I noticed three salmon affected out of the seven, and in a much worse condition—being all marked from head to tail; and this morning, on again examining them, I found the fourth slightly affected, making now only three out of the seven clear of the supposed disease.—I am, Sir, your most obedient Servant,

"JOHN NICHOLSON, *Inspector*.

"To Mr Superintendent SEMPILL,
"County Constabulary, Carlisle."

Each of the salmon mentioned in Nicholson's report had a label attached to it, stating when it was free of fungus, and when first observed to be affected, as follows:—

No. 1. A male kelt, 8 lbs.—"Observed slightly affected on 16th January."

No. 2. A large male, 30 lbs.—"Was free of fungus on the 16th January, and was seen to be slightly affected on the 20th January."

No. 3. A female, 14 lbs.—"Was free of fungus on 20th January, and was observed to be affected on the 2d February."

No. 4. A male kelt, 9 lbs.—"Was free of fungus on 2d February, and was observed to be affected with fungus on the 3d (or following day."

All those salmon were carefully examined—both anatomically and also microscopically. They were found to be affected with *Saprolegnia ferax* in various degrees of intensity over the whole body. The viscera and organs of generation were perfectly normal, and a number of valuable preparations have been added to the Museum which were prepared from them.

Inspector Nicholson's observations are very valuable, showing not only the sudden attack and rapid growth of the fungus upon the fish, but also that frost and ice have no effect in either checking or destroying the growth and spreading of the plant, as has been stated by Mr Buckland. The salmon noticed by Inspector Nicholson to

be free of disease on 20th January was a female, and was frozen over for twelve days—from 20th January to 2d February—during which period she had been attacked by the fungus, which had spread over her from head to tail. It is also worthy of remark that this fish had spawned while under the ice. Some of the ripe ova were found loose in the abdomen when I opened her for examination, and from the fact that one of the males frozen over along with her was in a condition to impregnate the ova, thousands of them may have been fertilised.

The fourth theory, that the kelts are diseased, and in consequence are first attacked by the fungus, and communicate it to the clean fish, I conceive to be no better founded than the theories of pollution, overcrowding, and absence of frost. In support of this opinion I quote from Mr Buckland's report, page 11, a statement by Inspector John Nicholson of the Eden district, in which he says—"That the total number of fish buried by the police since the 1st of March last is 1451. Between Armathwaite and Sandsfield there were buried 1271 salmon, 40 brandlings or parr, and 140 fresh water trout (*Salmo fario*), and in tidal waters of the river 100 salmon. The greater part of the 1271 above mentioned salmon were clean fish, having every appearance of having died of the disease so prevalent in the Eden at the time; about 50 were found at the river side in a dying state, which were killed and buried. About 200 were unclean salmon or kelts, which showed no symptoms of the disease; the brandlings and trout were all diseased." We have in this report evidence, and that on a large scale, that of the salmon buried by the police in the Carlisle district about 1000 affected with fungus disease were clean fish; the 200 kelts were not affected by the disease. Again, since I began to investigate the fungus disease, I have received, either from the Tweed or the rivers draining into the Solway, 16 salmon, 9 of which were clean fish. The addled ova suggestion is scarcely worthy of notice. There is no evidence that a salmon redd has ever been seen in any river in a state of fungoid growth, or that the fungus, if so grown, is the *Saprolegnia ferax*. Further, there is no evidence that the fungus which grows upon the carcases of dead kelts, which hitherto have been allowed to rot in the river, is the *Saprolegnia ferax*. The statement that this form of disease has been known in the Tweed for fifty years.

has, so far as I know, not been supported by scientific evidence, and rests mainly on the recollections of old anglers and fishermen.

I shall now proceed to relate observations on the disease as it has shown itself upon the salmon and other fish of the river Tweed.

On 12th April 1879, I received from G. H. List, Esq., chief-constable of Haddington and Berwickshire, three salmon, which were taken alive from the Tweed on 11th April. They were captured at Cornhill boat fishery, near Coldstream. All the fish were extensively affected with fungus on all parts of their bodies and fins.

The fungus is identical with that found upon the salmon and other fish of the Solway rivers described by me in 1878.

No. 1, a female salmon.—This fish was in the act of spawning when captured; complete dehiscence of the ovaries had taken place, and the greater part of the ova were shed, about six ounces, by measure, being retained in the cavity of the abdomen. The germinal membranes of the ovaries were plentifully supplied with germs for the following season.

The condition of this fish as a spawning baggit was very good; the skin, where not covered with fungus, was clear and silvery; the gills were high coloured and free from parasites of any kind; all the viscera were healthy, and a fair amount of fat adhered to the stomach and pyloric cæca. Blood taken from the heart, liver, spleen, and kidneys was carefully examined under the microscope, and was found to be perfectly normal. The lower part of the intestine was filled with a semi-transparent mucus of a pale rose colour, in which bacteria were very numerous. Two tape-worms of large size filled a considerable portion of the intestine with their plicated folds.

This salmon had over a dozen large patches of fungus adhering to it; one of them was 4 inches long by 3 inches broad, and was felted to one-fourth of an inch in thickness in the centre, forming a limpet-like crust of a slatey-grey colour. On careful removal of those patches of fungus, in most instances only a discoloured mark corresponding to the patch was seen adhering to the outer surface of the scales, which were in their normal position; but in several of

the thickest patches some of the scales were loose, and an extravasation of blood had taken place within the dermal sacs. On microscopical examination of this blood it was observed to be quite granular, containing no discs, and was freely mixed with oil globules. The under surface of the skin opposite to the patches where the scales were loose was inflamed and discoloured over an area larger than where the scales were loose, the tendinous attachment of the muscles to the skin was intact, and the muscles themselves were uninjured in any way.

No. 2, a male kelt.—This salmon had twenty patches of fungus on its body and fins, and its mouth was quite filled with it. The fungus on this fish was very rank; several patches were felted to five-eighths of an inch in thickness. The felting is caused in the first place by the filaments being twisted upon themselves and overlying each other, which prevents the spores escaping from the zoosporangia at the apex of the filaments in which they germinate and grow, and by the filaments sending out innumerable delicate fibres, which are woven by their own growth and the action of the water into a thick mat, in which the detritus of the river becomes embedded. The blood, mucus, and fæcal matter of this fish were examined in the same way as in the female, showing the same results. Several teniæ were found in the intestine, otherwise the viscera were quite healthy.

No. 3, a kipper grilse.—This salmon had twenty-four separate patches of fungus on its body, fins, and head, also a large patch seated on the mucous fold of the mouth, and involving both upper jaws and palate, which would cause difficulty of breathing, and the growth continuing would cause death from suffocation.

This fish looked a decided kelt, and was labelled as such by Inspector Johnston of Coldstream. However, on opening the abdomen I was surprised to find that the testes were not fully developed. The pyloric cæca and intestines were loaded with fat, and all the viscera were in a healthy condition, and, on the most minute and careful examination, nothing indicating disease of any of the organs could be detected. Externally this fish was disgusting to look on, as, in addition to the numerous patches of fungus on its body, several cicatrices on the head from former injuries gave it a most repulsive expression.

I also examined two salmon taken at Berwick-on-Tweed, at one of the Berwick Fisheries Company's stations, situated in the tide-way about one mile from the sea. One of those salmon I received from Sir Robert Christison, the other from Mr G. L. Pauline, secretary to the Berwick Fisheries Company. Both of those fish were affected with fungus, and were injured about the head and fins in a similar manner to those taken miles above the tide-way; the fungus also presented all its characteristic features. Both specimens were maiden salmon, and in excellent condition. They were both cooked, and were partaken of by sixteen persons, twelve of whom were fully informed that the fish were affected with fungus, four persons were not informed until after they had digested it. The former, myself included, all say that they knew nó difference in either taste, colour, or smell from fishmongers' salmon; the latter say that I am only trying their nerves in saying the fish had fungus disease, as they had never eaten better.

I have also examined two other salmon, both of which were maidens. They were both from the Tweed river. One was presented to me by Mr Speedie, gamekeeper at the Inch, who saw it in a dying state and pushed it out of the river with a stick. This was a beautiful salmon, with only a few patches of fungus on its body and tail, which were easily rubbed off, leaving only a slight stain of a brassy hue where they were seated, and with the scales intact. A very large patch was seated within the mouth, involving both the upper and lower jaws, the palate and mucous fold in the upper part of the mouth, and extending to the gills, which were also thickly studded with parasitic crustaceans, from the combined effects of which it was dying of suffocation.

The other salmon was presented to me by Arthur Campbell, Esq., Randolph Crescent, Edinburgh. It was captured in the Tweed at Maxton. This fish was a maiden salmon, in high condition, and exceedingly fat and firm. It was injured about the forehead and nostrils from fungus having been seated upon them. The frontal bones were exposed, and appeared as if corroded by friction, and the skin around this part had begun to slough; no fungus adhered to the bare part of the bone, but the loose skin surrounding it was thickly coated with it. There were a number of patches of fungus on, its body and fins, but no sloughing had taken place under them. I

have injected and preserved the stomach and pyloric cæca of this fish as examples of those organs in a high conditioned fish. It was cooked, and several gentlemen who partook of it pronounced it excellent.

By the kind attention of James Tait, Esq., of Kelso, I received a common river-trout and a minnow, both of which were captured near Kelso Bridge in Tweed river; both specimens were affected with fungus—the *Saprolegnia ferax*. I may here mention that I have noticed several able letters which have appeared in the *Scotsman* newspaper from time to time, in which the writer states that the fungus is only a secondary attack, and that a primary disease of an inflammatory kind first affects the head and other parts of the salmon before the fungus can settle upon it. I do not for an instant doubt the fact that the writer saw fish with sores of the kind described by him upon them, when there was no fungus present to cause them. I can only say that, among all the fish which I have received for examination, consisting of salmon, sea-trout, smolts, common trout, greyling, and minnows, I have not seen one with a sore on which this fungus was not present; while on every fish examined there were some patches of fungus which could easily be wiped off, leaving only a slight stain, and in some instances no mark could be discerned, and no loosening and shedding of the scales or ulceration of the subjacent surface. Again, in every instance where the fungus was rank, long-seated, and felted, sores in every degree, from slight abrasion to sloughing, were found under them. With reference to the trout and the minnow before mentioned, the trout had fungus seated upon the gums of both the upper and lower jaws, which involved both the teeth and lips, and had spread upward and backward upon the head, and its destructive progress could be easily traced: first, the skin of the lips was broken in several places, and shreds of it were hanging loose, to which the fungus was adhering; while, as it spread backward over the nostrils and crown of the head, the skin and its pigment spots could still be seen intact where the fungus was seated, a portion of which had been carefully shed aside to expose the skin. On each of the pectoral fins a patch of young fungus was seated, and the mucous coat was seen through the fungus to be quite entire; the same appearance was seen upon the anal fin and

scaled parts of the body. The minnow had only one patch of fungus upon it, which was seated within its mouth on the inner margin of the right lower jaw; it filled the mouth, which was distended by its growth; and every other part of its body was free from fungus or blemish of any kind.

The reason why most of the fish affected with fungus are first attacked by it upon their heads may arise from various causes. All river fish present their heads to the downward current of the water whether they are swimming or at rest, and as the spores of the fungus are floating down with the stream, the heads of the fish are the first parts to come in contact with and be affected by them. Further, the mucous glands are most numerous and active upon the head of the fish, which is also more thickly covered with mucus than other parts of the body, and the spores which fall upon it adhere more readily; and the fins and tail, from their continuous waving motion, are more liable to arrest the passing spores than the parts of the body from which they spring, and, from this cause, are generally affected sooner than the bodies of the fish.

The numbers of the dead and dying fish of all kinds removed from the river Eden in 1878 by the police, and published by Mr Buckland in his report for that year, show that there were 1271 salmon, 140 fresh-water trout, and 40 brandlings or parr, being over 50 of the large fish to every one of the smaller. About 1000 of the salmon were clean fish, and it may be inferred that the trout and parr were also clean, which goes far to show that the so-called disease is as much a mechanical as a functional one. Further, from documents descriptive of the effects of the disease in the river Tweed, in the lower district, during this season 1879, which were collected by the police from taxmen and practical fishermen on the river, I find that the proportion of large fish affected, dead or dying—namely, salmon and sea-trout—is very great compared with the smaller fish, which were found to be affected in a similar way. The smaller fish alluded to consist of river-trout, greyling, smolts, perch, and grey mullet.

From observation of the fungus and of the fish affected by it, I am led to believe that the so-called salmon disease does not depend upon a pre-diseased condition of the fish. It is a true parasitic attack to which every fish in any affected river seems to be liable,

as every kind of fish, irrespective of condition, appears to be a proper nidus for the propagation of the *Saprolegnia ferax* when a living spore from that fungus attaches itself to it. While engaged during the spring and summer in the microscopic examination of the *Saprolegnia ferax*, I observed that as the season advanced many of the patches of fungus seated upon the fish were barren, consisting of spear-shaped filaments only, having no zoosporangia at their apex, and consequently they produced no zoospores; the filaments were long and very thin, and almost void of protoplasmic contents, indicating that the plant was losing its force and in a state of decay.

The *Saprolegnia ferax*, in all probability, is always present in our rivers in more or less active condition. It is believed that this fungus has two modes of reproduction, namely, by oospores and by zoospores. The oospores are few in number, and may be looked upon as ova, and they require sexual impregnation. They are called resting spores, from a belief that they remain dormant in the water for an indefinite period, which may continue for many years; and during this phase of their life they may germinate in limited numbers, providing only for the continued existence of the species. While in this state of abeyance there is no plague of fungus, from the ova only producing neutral or barren plants, which bear no fruit or seed. After a period, of longer or shorter duration, a season, or a series of seasons, may follow, during which an unknown influence arises, which acts upon the resting spores, by which they are stimulated to great reproductive energy; and the plants they produce being fruitful, the asexual mode of reproduction commences.

The zoospores are produced in podlike cases called zoosporangia, which are situated at the apex of the filaments, and may be looked upon as fruit or seed. They are the ciliated spores, and are the media by which the fungus is communicated to the fish. The zoospores are produced in great numbers, each zoosporangium containing from 100 to 150 of them. The oospores or ova are produced in a globular sac, which forms at the root ends of the filaments, or upon the roots themselves. Those sacs are called oogonia, and each sac contains a few oospores or ova, three or four, to nine, being the numbers I have observed in the four instances in which I have seen them, in the whole course of my investigations.

Suppose an oospore (resting spore) to be capable of producing, under favourable circumstances, a plant carrying 100 filaments, and each of the filaments to produce 100 zoospores, 10,000 germs would be derived from a single ovum or resting spore, every one of those germs being capable of producing a plant as productive as that from which it derived its existence, a multiplication of innumerable millions would be produced in a few days, the ciliated spores being as plentiful in the water as snow-flakes are in the air during a snow shower; and in this way the plague of fungus, the so-called salmon disease, is originated.

I obtained in April the living fungus from a greyling caught by Mr J. Willins, student of medicine, when angling in Keerfield Pool in the Tweed, near Peebles. It had been cut in two halves and the tail portion selected; it was packed in a tin vessel with wet moss, which had preserved the fungus in active vegetative growth, when I received it on the morning after its capture. A pale pink bloom was plainly visible over the whole surface of the matted fungus, and, when it was held up between the eye and the light, a new growth appeared to cover the older fungus on its outer surface to about one-eighth of an inch in height.

When examined under the microscope in water, free ciliated zoospores, which had escaped from the zoosporangia situated at the extremities of the filaments, were observed in motion—they moved in a fitful way, by short jerks, not by a continuous movement.

Those zoospores were pyriform in shape during the short time they were observed in motion; on becoming stationary the cilia disappeared, being probably withdrawn into the body of the spore, which then assumed a globular form. This change took place in a very short time—not exceeding ten minutes,—and while under observation minute projections became visible on the edge of the spore, which grew into delicate filaments of considerable length. I have succeeded in fixing the development of the fungus in this state, and it can be seen in various stages of growth, all of which were ciliated spores within the space of one hour.

This, the asexual mode of propagation, is remarkable for the rapidity with which it is accomplished. A few of those ciliated spores become attached to any part of either a healthy or a diseased

fish; in one hour the cilia will have disappeared and a filament of some length will have sprouted from the spore. Thus, in a single day, a fish, on which no fungus could be discerned, is to-morrow seen to be affected, and in three days is spotted or patched over with fungus from head to tail.

In the second or sexual mode of production of spores a short pedicle is pushed out from one of the sides of a filament on which a globular sac—oogonium—is formed, and within this sac a number of oospores are produced, which are spherical in shape and have a cell wall or envelope, and some are provided with a nucleus in the centre. These, after impregnation, escape from the oogonia, and are probably capable of living in the water for an indefinite period, in a dormant or resting state, until the conditions arise which are favourable for their germination.

It may be asked, how does the fungus affect the fish, and do any recover from its effects? The fungus produces a local irritation and inflammation of the integument, as is evidenced by the congestion, and even ecchymosis of the true skin, by abrading of the scales, and in the more advanced stages by ulceration and sloughing, affecting the whole thickness of the integument and mucous surface.

Wherever the fungus adheres and spreads, the function of the skin is necessarily interfered with. Light, which is so essential to the fish in promoting its pigmentary secretions, is cut off from a large portion of its skin. Endosmosis, exosmosis, and the secretion of the mucus for lubrication are destroyed, and in this way constitutional symptoms would be occasioned which, if the disease continued, lead to the death of the fish.

The second question, Do any fish recover from fungus attack? may now be answered more hopefully. The fishermen and watchmen on the Tweed report having seen several fish with new skin growing over the sores upon their bodies, from which this fungus had disappeared, and I am inclined to believe that this is so. A male kelt has been sent to me by Mr List, which was taken in tidal water below Berwick bridge. This fish is 2 feet in length, and weighs about three or four pounds; it is supposed to have been affected with fungus, and to have completely recovered from its effects. No particle of fungus could be found upon any part of its body, and

there was only one raw sore. This sore was only $\frac{5}{8}$ths of an inch in length and $\frac{3}{8}$ths in breadth. It had evidently been larger, and had a smooth healing border. All the upper surface of the head and snout were covered with skin, but very uneven over its whole surface, from depressions and projections which may have been caused by sores which have been healed over, and the hinder part of the operculum had an irregular cicatrix of considerable size upon it. The breast and belly, from the gill coverts to the vent, were blood-streaked and spotted, and there were brownish marks upon both its back and sides as if fungus had recently adhered to it. All the fins were entire,—not one ray was broken; and the fish as a whole looked remarkably well for a kelt, and if it had been affected with fungus, which I fully believe, its recovery has been almost perfect.

A salmon taken at some distance up the river, and which is affected with fungus, has been taken down to Berwick and placed in a box or corve, and is now anchored in the river, in the tideway, where the water is at all times less or more salt, and at intervals is towed out to sea, where the full influence of the salt water acts upon it; and when I last heard of it considerable improvement had taken place. Mr G. H. List has paid particular attention to the detection of any fish being affected with fungus disease in any of the coast fishing stations; and, after the most careful inquiry, no trace of any fish in the least degree diseased at any of those stations could be got, nor, as far as any fishermen either knew or heard of, was any salmon with fungus upon it ever seen in salt water.

I have tried to propagate this fungus upon dead flies, spiders, and other small animals, following the directions of Pringsheim, "N. A. A. L. C.," 1851, p. 417,* who says—"All that is required to obtain a living specimen of this singular plant is to allow the body of any small animal, such as a fly or spider, to float for a few days in rain water exposed to the light. By this method a crop of *Saprolegniă* may be obtained at any season." In this way I got a fungus upon the flies and spiders after an exposure of from twelve to twenty days, which on examination was found to be a common

* Cited by Dr Burdon Sanderson in his paper on the "Vegetable Ovum," *Cyclopædia of Anatomy and Physiology*, edited by Dr Todd.

mould exactly similar to that produced upon a solution of gum-arabic, gelatine, and meat infusions. I have tried to propagate the *Saprolegnia* fungus upon minnows, but without success hitherto, doubtless because the method adopted did not provide the proper means, there being awanting the necessary stimulus which exists in the river, or, what is more likely, the life of the fungus itself. The minnows were placed in a large glass vessel filled with town water from the tap. A piece of skin with this fungus adhering to it was taken from a salmon smolt and placed in the water along with them. In three days they had eaten up both skin and fungus, and remained unaffected. Several large patches of this fungus were then taken from the skin of a salmon and placed in the vessel along with them. In a few days it had all disappeared, and produced no effect. Another method was suggested by Mr G. H. List, who also kindly furnished me with material for the trial. Pieces of skin with this fungus growing upon them were cut from the bodies of dead salmon at the river side, and were put into wide-mouthed bottles, which were at once filled with river water, the skin not being allowed to dry. On receipt of the bottles the pieces of skin, along with the water in which they were brought, were emptied into the vessel among the minnows. The water in the vessel was not changed for three days, at the end of which time the minnows were still unaffected. Fresh water was then put in the vessel, and the pieces of skin retained in the water, which was changed every second day for eight days. The minnows were not disturbed by the pieces of skin. They nestled under them and nibbled every morsel of fungus from them, hiding and playing about them until they had to be removed from putridity. All the minnows are still alive and are in beautiful condition, taking food greedily, worms cut small and crystals of sugar being their favourites. They have been kept since 14th May till now, 12th July, and are as healthy and lively as when put in the vessel.

I have received from Thomas Key, Esq., Fellow of this Society, some information respecting a disease affecting the salmon in Lewis some years ago, which, from the sores of the skin of the head, resembled at a first glance a condition not unfrequently found on salmon affected with *Saprolegnia*. After examining my specimens, Mr Key wrote to me the following account of the disease in the

Lewis, which is of so much interest in connection with this subject that I append it to my communication :—

"From what I saw and heard from you, I am convinced that the disease from which the salmon in the Grimasto river in Lewis suffered some eleven years ago differed from the one you are now investigating. In the first place, we had *no trace of fungus*,—the affection was confined to the head, and although it destroyed many fish yet very many recovered from it. It attacked the fish in the sea, or, according to my theory of its origin, in the brackish water between the sea and the mouth of the river. It assuredly had not its origin in the river, or in the loch above it. There are many brown trout in our rivers and lochs, and none of them suffered. Neither among the multitudes of sea trout about us did I see one affected. The disease I am speaking of appeared about the middle of the season of 1868. As happens frequently in the Lewis, the months of May and June had been very dry, and for weeks before rain came, some time late in July, the fish had not been able to get up the river in consequence of want of water at its mouth. We were told that many fish had been found dead in the bay, and after rain had fallen and we were able to fish in the river and lochs, we then saw the nature and extent of the disease. Fish were found dead and dying in the river and at its mouth; others not too far gone, took the fly, and were caught. On the dead fish examined the whole of the upper part of the head was found covered with ecchymosed spots and ulcerated, the ulcers more or less superficial, and some with everted edges. In some the cartilages of the nose had been attacked, and one side of it cut out as it were by a corroding sore. When cut into, the bones and cartilages of the head were found to be softened, and there were marks of inflammation in the brain and membranes. The eyes were natural, the gills pallid but otherwise sound, and none of the fins affected. In the far advanced cases, among the fish caught, the softened appearance was very much the same, whilst in those less diseased the ulcers were few and small, the rest of the head being simply ecchymosed. In a great many fish recovery apparently soon commenced, the ulcers began to cicatrise, and the fluid in the ecchymosed spot was almost altogether absorbed. In every case, I may say, we observed the gill covers had a dull, white, leprous appearance, and in all the fish

that recovered the head was more or less *white*, and continued so for years afterwards; and even to this day, every now and then, a white head, as the gillies called the diseased fish, comes up from the sea.

"It is a curious and interesting fact that the condition of the fish was not affected in even the far advanced cases. Nutrition did not appear to have been interfered with. The body was as plump and fat and the pink colour as high as usual. I did not eat of those very far gone in the disease; of those less so I did eat, and found their flavour as in the healthy salmon. You will observe from what I have said that our disease, whatever might have been its cause, was a disease of the head, and confined to the head.

"So much for the form of our disease; now as to its origin. Whatever may have been the predisposing or its immediate cause, it is certain that the fish brought it with them from the sea, or, as in my opinion, acquired it in the tide-way in Loch Roch-Roag. They did not take it down with them when they went to the sea as kelts or smolts, but they brought it up from the sea in summer as grilse and fresh-run salmon. After mature consideration of all the attendant circumstances, I have come to the conclusion that the disease arose from the fish being kept moving so long up and down between the salt and brackish waters. With each flood tide they moved up in dense masses toward the mouth of the river, vainly looking for water sufficient to carry them into it, and, when the ebb came, going down again for two or three miles into the deep and comparatively salter water. This continuing for weeks, with the water in the bay becoming daily more shallow, the heat and bright sun during the day was sufficient, in my opinion, to account for the disease. I have already said the sea trout did not suffer, because very little water was sufficient to take them into the river, and they were kept outside for but a short time. Again, the fish in the Blackwater, a river within two miles of the Grimasto, had no disease—at least, I did not hear of any having been seen in it; the reason, as I think, being that at all times, except in the lowest neaps, the tide came up so near its mouth as to allow the fish to get into its lowest pools without much difficulty. Against my theory there is this to be said: as already mentioned, the island of Lewis has been subject within the last fifteen years, to my knowledge, to

many dry seasons; and notably in 1863 there was a very long-continued drought, proving so destructive to the salmon at Grimasto that it was said some 1500 fish were picked up dead on the shores of the bay and mouth of the river. I and my friends were not in the Lewis that year, and therefore I cannot speak as to the symptoms of that disease; but inquiry afterwards failed to elicit any evidence that they resembled the outbreak of 1868. . . . It may be that the fish in 1868 were in some peculiar abnormal condition *before coming up from the sea*, predisposing them to disease of the head; but at any rate I can give no other cause for the outbreak than those I have mentioned."

3. On the Form and Structure of the Teeth of *Mesoplodon Layardii* and *Mesoplodon Sowerbyii*. By Professor Turner, M.B.

The author in the first instance described the characters of the teeth of *Mesoplodon Layardii* from two specimens which had been collected during the expedition of H.M.S. "Challenger," under the scientific superintendence of Sir C. Wyville Thomson. The one specimen, a young animal, under 14 feet in length, was obtained at Port Sussex, East Falkland Islands, by Mr H. N. Moseley, F.R.S.; the other, an adult skull, was procured at the Cape of Good Hope.

The teeth of the younger animal, two in number, were imbedded in their alveoli in the lower jaw. Each tooth consisted of a small triangular denticle or crown projecting outwards, and slightly upwards from the middle of the upper border of the fang. The denticle measured 4-10ths inch in its longer diameter, the fang was 2 inches by 8-10ths. At the base of the fang was a cleft 2-10ths inch wide, which communicated with a pulp cavity that was prolonged almost to the apex of the denticle.

The denticle was invested by enamel, subjacent to which was a well-defined mass of dentine, which was prolonged as a thin layer almost down to the cleft at the root of the fang. The fang was invested by cement, which was separated from the dentine

by an opaque layer, consisting of a granulated matrix containing numerous branched and anastomosing vascular canals, like the Haversian canals of bone. A similar layer was prolonged into the pulp cavity, so as to line the dentine on its inner surface. This layer is apparently to be regarded as a modified form of vaso-dentine.

The teeth in the adult mandible were formidable tusks, which curved up the sides of the beak on to its dorsum, where they decussated across the middle line. Each tooth was 14 inches long, $7\frac{1}{2}$ inches of which had protruded beyond the gum. It consisted of a triangular denticle and a strap-shaped curved shaft. The denticle was somewhat smaller than in the young tooth, and the enamel was almost entirely worn off its surface. The size of the tooth was therefore due to the enormous development of the fang which formed the strap-shaped shaft. The shaft consisted for the most part of a cortical layer of cement investing an opaque central band, which had the structure of the modified vaso-dentine of the younger tooth. 7-10ths of an inch from the summit of the shaft was a minute mesial chink 1-10th inch long, which represented the pulp cavity, but the rest of the shaft was solid throughout. The summit of the shaft was more complicated in structure, and consisted from without inwards of the following layers :—cement, opaque modified vaso-dentine, opaque vaso-dentine, dentine, opaque modified vaso-dentine. When traced from the summit to the sides of the shaft the dentine and vaso-dentine disappeared, and then the two layers of modified vaso dentine blended with each other and formed the opaque central band of the rest of the shaft. The size of the fang is due to the great growth of the cement and the tissue of the opaque central band. The teeth of this specimen are larger than in any of the previously recorded specimens, and the animal from which they are obtained was probably an old male.

The structure of the teeth of *Mesoplodon Sowerbyii* was examined from the skull described by the author in the "Transactions of the Royal Society, Edinburgh," 1872, vol xxvi. Each tooth was laterally compressed, and formed almost an equilateral triangle, and the crown was not separated from the fang by any sharp line of demarcation. The tooth consisted in great part of dentine, which in the crown was invested by a layer of not very strongly marked enamel.

The dentine extended down the fang to the sides of the narrow cleft at its base, which communicated with the pulp cavity. This cavity, bounded by the dentine, was contracted at the base of the fang, but dilated into a considerable space in the body of the tooth. The fang was invested by cement, but between the cement and dentine a layer of modified vaso-dentine was situated which increased in thickness in the lower part of the fang, whilst the dentine became thin. The structure of this tooth was then compared with that of the adult *Mesoplodon Sowerbyii* described by Professor Ray Lankester.*

The teeth both of *M. Layardii* and *Sowerbyii* in their non-erupted stage do not materially differ in structure from the ordinary human or carnivorous teeth, for the crown is covered by enamel, and the fang by cement, whilst the great body of the tooth consists of dentine, in which is a well-marked pulp cavity. The exceptional structure of these teeth in the erupted stage is due to the disappearance of the enamel from the crown, to the cessation in development of the ordinary dentine, and to the excessive formation in the adult *Sowerbyii* of osteo-dentine, and in *Layardii* of modified vaso-dentine, which cause the fang to assume unusual dimensions.

The following Gentleman, having been duly recommended and balloted for, was elected a Fellow of the Society:—

James Abernethy, V.P. Inst. C.E., Prince of Wales Terrace, Kensington Garden, London.

Monday, 16th June 1879.

Professor MACLAGAN, Vice-President, in the Chair.

The following Communications were read:—

1. Atomicity or Valence of Elementary Atoms: Is it constant or variable? By Professor Crum Brown.

* "Quarterly Journal of Microscopic Science," 1867, vol. vii.

2. Action of Heat on some Salts of Trimethylsulphine. By Professor Crum Brown and J. Adrian Blaikie, D.Sc. No. IV.

I. The carbonate of trimethylsulphine is obtained by the action of carbonate of silver on the iodide of trimethylsulphine. The solution of the salt may be evaporated to a syrup in the water-bath. On standing for some weeks over sulphuric acid *in vacuo* it crystallises out in exceedingly hygroscopic prismatic crystals, containing water of crystallisation, and having a strong alkaline reaction.

Heated in the air to 100° the salt gives off water, sulphide of methyl, and carbonic acid. Heated in a sealed tube to 100° C. for about eight hours it was almost entirely decomposed, gave off a gas consisting entirely of carbonic acid, and yielded two layers of liquid —the upper, sulphide of methyl; the lower, water and methylic alcohol. The decomposition is expressed by the equation—

$$\{(CH_3)_3\}_2CO_3 + H_2O = 2(CH_3)_2S + CO_2 + 2(CH_3)OH\ .$$

II. The metaphosphate of trimethylsulphine is obtained by the action of metaphosphate of silver on the iodide of trimethylsulphine. The metaphosphate of silver was made from glacial metaphosphate of soda (Graham's salt) by precipitation with nitrate of silver. The metaphosphate of trimethylsulphine does not crystallise, but on evaporation leaves a colourless hygroscopic glass, containing some water.

The salt, when acted upon by heat, gives off sulphide of methyl, and the resulting product is at the same time decomposed, leaving free metaphosphoric acid. On further heat being applied the mass slightly chars.

III. The ferrocyanide of trimethylsulphine is obtained by the action of ferrocyanide of silver on the iodide of trimethylsulphine. On evaporation of the solution the salt crystallises out in pale-green transparent plates; they are not hygroscopic, and the salt gives all the reactions of an alkaline ferrocyanide. On drying over sulphuric acid or phosphoric acid, the crystals lose their water of crystallisation. Analysis leads to the formula $\{(CH_3)_3S\}_8Fe_2Cy_{12} + 18H_2O$.

The salt when heated to 220° C. gives off sulphide of methyl along with other products, including hydrocyanic acid, but does not

melt. The residue is a brown powder, which on being further heated is carbonised, no definite compound being obtained.

IV. The ferricyanide of trimethylsulphine is obtained by the action of ferricyanide of silver on the iodide of trimethylsulphine. On evaporation of the solution the salt crystallises out in pale orange-yellow transparent plates, which effloresce in the air. The salt gives all the reactions of an alkaline ferricyanide. On drying over phosphoric acid the crystals lose all their water of crystallisation. Analysis leads to the formula $\{(CH_3)_3S\}_6Fe_2Cy_{12} + 15H_2O$. The salt when heated behaves similarly to the ferrocyanide.

3. Comparison of the Salts of Diethylmethyl-sulphine and Ethylmethylethyl-sulphine. By Professor Crum Brown and J. Adrian Blaikie, D.Sc.

(*Abstract.*)

It seemed to the authors to be desirable to ascertain the mode in which the salts of diethylmethyl-sulphine and ethylmethylethyl-sulphine respectively decompose when heated.

They prepared the iodides by the method described by Krüger,* whose observations on the iodides and chloroplatinates they substantially confirm.

The benzoates were prepared from the iodides by action of benzoate of silver. They are exceedingly soluble substances, and were only obtained as thick syrups. Heated to between 110° and 120° C. they decompose in exactly the same way, yielding benzoate of methyl without any benzoate of ethyl.

4. On the Bursting of Firearms when the Muzzle is closed by Snow, Earth, Grease, &c. By Professor George Forbes.

It is well known that if an ordinary fowling-piece, charged with shot or ball, have touched the ground or snow, so as to close the muzzle of the gun, or if the muzzle of the gun be in any way artificially closed with grease or other substances, the fowling-piece is certain to burst at the muzzle when it is discharged. This would not be the case if, instead of firing a shot, a piston were driven up the tube by hand. In this case the compressed air would drive out

* *Journal für practische Chemie*, xiv. 193–213.

the opposing plug, which offers but a very feeble resistance to the internal pressure. These facts, thoroughly well authenticated, have not, to my knowledge, received a satisfactory explanation, though a clear idea of the conditions of the case is all that is required to explain this, at first sight, anomalous behaviour.

The explanation lies in the fact that the charge travels along the bore of the gun, if not with the same velocity as, at least with a velocity comparable to, that of the transmission of pressure through the air, *i.e.*, the velocity of sound. Thus, as the charge advances along the barrel it is continually compressing the air immediately in front of it; but this pressure gets no relaxation by expansion into the front part of the barrel. The compression, of course, generates heat in the air, which increases the velocity of sound through it. But this does not affect the question in its general bearings. It is sufficient to notice that the snow, &c., is driven out with the full velocity of the charge (neglecting the weight of the snow-plug compared with that of the charge). But before the plug can be driven out with this great velocity the pressure behind it must be very great.

Let m = the mass of the snow-plug.
g = the force of gravity.
v = velocity of the bullet or wad when close to the plug (*i.e.*, on leaving the gun).
p = the pressure of the air driving out the plug.
A = the sectional area of the bore.
b = the length of the snow-plug.
ρ = the density of the snow-plug.

The work done in giving to the mass m a velocity $= v$ is

$$w = \frac{1}{2} m v^2.$$

But w is performed by the pressure pA acting through the distance $\frac{b}{2}$.

$$\therefore \qquad w = \frac{1}{2} pA \cdot \frac{b}{2}.$$

$$\therefore \qquad pAb = mv^2.$$

$$p = \frac{mv^2}{Ab} = \rho v^2.$$

Thus, the pressure at the muzzle of the gun is independent of the diameter of bore and length of plug.

To take a particular example, let $v = 1000$ feet a second, and let $\rho =$ the density of water, so that

$$p = gh\rho$$

when h is the height of the column of water producing an equal pressure—

$$gh\rho = \rho v^2$$

$$h = \frac{v^2}{g}.$$

If w be the weight of a cubic foot of water, $p = wh = w\frac{v^2}{g}$ is the pressure on the square foot.

Now, $w = 72$ lbs. and $g = 32$ and $v^2 = 1{,}000{,}000$;

$$\therefore \quad p = \frac{72}{32} \times 1{,}000{,}000 \text{ lbs. on the square foot;}$$

$$\text{Or,} \quad \frac{1}{144 \times 2240} \times \frac{72}{32} \times 1{,}000{,}000 \text{ tons on the square inch}$$

$$= 7 \text{ tons on the square inch.}$$

A pressure which the muzzle of a shot gun is not constructed to withstand, and the theory shows that this great pressure can be produced even by a plug of snow or grease of the shortest length movable inside the barrel with the greatest facility. If the velocity of the ball or wad be less than that of sound the snow-plug is not driven out quite suddenly, and if the velocity be small enough the snow-plug is driven out before the ball or wad reaches the muzzle.

5. On some New Bases of the Leucoline Series. Part III. By G. Carr Robinson and W. L. Goodwin.

Monday, 7th July 1879.

Professor MACLAGAN, Vice-President, in the Chair.

The following Communications were read:—

1. Notice of Striated Rocks in East Lothian and in some adjoining Counties. By David Milne Home, LL.D.

I know no more interesting problem in geology than the question, What was the great agency which brought the surface of

northern Europe into the condition in which it is now occupied by man? and it seems marvellous that geologists should not yet be agreed as to what that agency was.

Our own country of Scotland is strewed with boulders, many of immense size, and which we allow have been somehow transported to their present sites from remote regions. Rocks on our hill-sides have been ground down, smoothed, and striated by ponderous bodies which have come against and rubbed upon them. Almost everywhere there are deep beds of clay, sand, and gravel forming knolls and elongated ridges, not only on low-lying districts, but even on our highest hills. These things have been attracting attention and provoking discussions for more than sixty years; but no explanation has yet been arrived at, which meets with general acceptance. Some geologists insist on the agency of an ice-sheet, like that in which Greenland is wrapped; Others stand up for local glaciers, such as exist in Switzerland and Norway. Some suggest icebergs and other forms of floating ice, in a sea which submerged the country. Each of these theories has its partisans; for no crucial test has been discovered to indicate which of them, or whether any, is well founded.

The Transactions and Proceedings of our Society contain many papers regarding these phenomena. Of these papers, the earliest probably was by Sir James Hall, so long ago as the year 1812, and he was followed by M'Laren, Chambers, Fleming, and many other Fellows of our Society, who specially devoted themselves to this branch of geological research.

The last paper published on this subject in our Proceedings, was by our colleague, Mr David Stevenson, who described a portion of the hill in East Lothian known as North Berwick Law, which was found by him to have been ground down, smoothed, and striated. These effects he ascribed to the agency of a glacier, which came from the westward against the hill, first smoothing the rocks on its north side by the heavy pressure of the ice, and afterwards scratching the smoothed surface by hard stones incased in and protruding from one side of the glacier. Mr Stevenson suggested that the glacier might even have been, and probably was, of such dimensions as to have enveloped the whole of the Law, which reaches a height above the sea of 612 feet.

At the close of his paper Mr Stevenson expressed an opinion

that if the rocks of Stirling Castle, Craigforth, and other places were examined, interesting and instructive traces of similar glacial action might be discovered.

To this suggestion of an inquiry for cases of a similar kind, I am now here to respond, and with that view to lay before the Society an account of several striated rocks in East Lothian, Stirlingshire, and other places; I feel sure that Mr Stevenson himself will deem these cases not the less interesting, though they should warrant conclusions different from those he suggested.

I. East Lothian Striated Rocks.

The first of these which I mention, as the least complicated, are in the village of Linton.

The rock is a claystone porphyry. Several smoothed patches of rock occur here, and two of these show striations on surfaces from 3 to 4 square feet in extent.

One of these smoothed rocks is *horizontal* or nearly so; and on it the striæ have a direction W.N.W. and E.S.E.

The other smoothed rock *dips* towards the north, at an angle of about 35°, and on it, the striæ run due *east* and *west*.

The difference between the directions of the striæ of these two rocks, which are only a few yards apart from one another, may be accounted for by the fact that the same agent which produced striæ in a certain direction on a *horizontal* surface, would, *if that agent could be easily deflected*, not produce striæ in the same direction on a *sloping* surface. It would have less power to move up an inclined plane, but would move along it more horizontally.

What the striating agent was here, and in what direction it moved, is made manifest by the following facts. Both patches of rock were, when I examined them, still partially covered by a coarse clay, full of stones or pebbles, many of which were hard and angular, but some were soft. There were among them bits of coal and limestone, which must have come from the westward, as in East Lothian there are no coal or limestone strata to the east of Linton. In one of the smoothed patches there were two small hollows or depressions, which had interrupted the continuity of some of the striæ. These depressions on their inner surface showed a vertical wall on their west side, and a sloping wall on their east

side, indicating that the smoothing agent had partially entered the hollow, and had worn down a portion of the east side.

Assuming that the general course of the striating agent here was from W.N.W. towards E.S.E., it is quite intelligible that when this agent, *if of a flexible nature,* impinged on a rock surface dipping to the north, its direction would change so as to be more to the eastward.

There is another rock of larger dimensions about a mile to the west of Linton village, on the line of the North British Railway. It is about 25 feet in length and about 18 feet in height, and

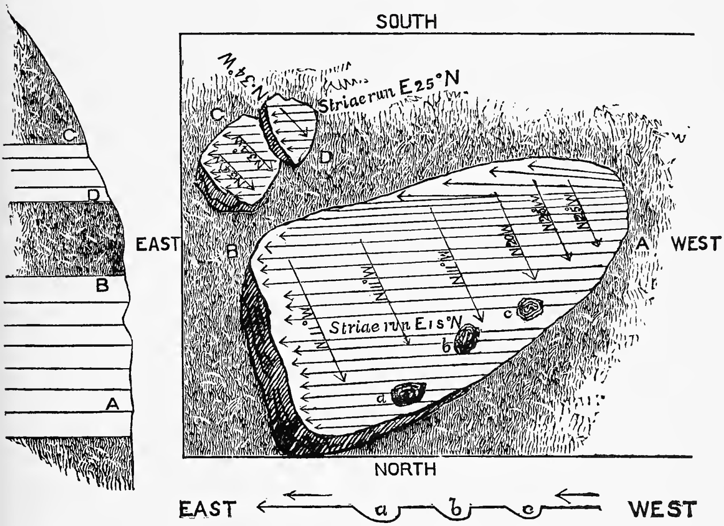


Fig 1.—Rock on Railway, near Linton.

presents a surface nearly vertical. It is on one side of a gully through which the railway passes, the rock being on the south side of the line.

The rock was discovered and exposed to view, when an excavation for the railway was made into a thick bed of boulder-clay which occurred here. The rock now seen had previously been entirely covered by the clay. With the consent and the assistance of the railway authorities, I had an additional portion of the rock at its west end stripped of the clay, to the extent of several square yards, when more smoothing and striation was brought to light.

Besides the rock which forms the lowest and principal part of

the bank, there is a small patch of rock, about 10 feet long by 4 feet wide, at the top of the bank, also smoothed and striated.

Both rocks dip rapidly, the upper one at an angle of about 55°, the lower one in its lowest part is vertical.

The principal rock is AB on the preceding diagram (fig. 1) and the smaller is CD.

The dip of each is indicated by the section at one side. The two rocks do not front exactly in the same direction. The lower rock fronts N. 11° W. for two-thirds from its east end; but near its west end it fronts about N. 30° W. The upper rock fronts about N. 34° W.

The striæ on both rocks are exceedingly numerous. There is not half a square inch on either without ruts or scratches. Some of the striæ on the larger rock are from 5 to 6 feet in length, and from $\frac{1}{4}$ to $\frac{1}{2}$ an inch in depth, and from 2 to 3 inches wide.

There is, however, a difference in the depth of the striæ which deserves notice and explanation. In the upper rock CD, they are much deeper and wider than they are generally in the lower rock. But in the lower rock, the grooves or ruts are deeper at the west end than towards the east. An explanation is suggested by the way in which the rocks front. If, as the rocks in Linton village indicate, the striating agent came from W.N.W., the obstruction to its progress eastward would be greater by a rock facing N. 34° W. than by a rock facing N. 11° W. Hence to overcome that obstruction, more pressure by the striating agent would occur, and deeper ruts in the rock surface would be made in the former than in the latter case.

It may be mentioned, that whilst generally the striæ on the large rock AB are horizontal, near the top they rise towards the east at an angle of about 4° or 5°. It may be supposed that if the striating agent consisted of a mass of detritus, the weight of the mass would keep the striating tools in the low parts of the mass in a line more or less horizontal, but that at or near the top of the moving mass, its component parts would rise, so as to obtain a direction more in conformity with the normal movement towards E.S.E.

In the upper rock, CD, there is a vertical joint, as shown in the diagram. It has had the effect of breaking the continuity of the striæ. The joint has a breadth of 6 or 8 inches, forming a face

which fronts N.N.E. There are no striæ on this face, as a force coming from W.N.W. would not strike on it.

In the lower rock there are small cavities or depressions (*a*, *b*, *c*) in the general surface, in consequence of which the continuity of the striæ has been interrupted. When I first examined the rock, two of these cavities were filled with boulder-clay. The west sides of the cavities are vertical, but the east sides are smooth and sloping, having apparently undergone attrition by the materials passing over them from the west. This point is further explained by the section EF at the bottom of the diagram.

Besides the smoothed and striated rocks in Linton and near it, just described, there are cases of the same kind at the following other places in East Lothian, all of which I have examined:—viz., North Berwick Harbour, Kingston, Dirleton, Redside, Balgone, Whitekirk, Smeaton, and Rhodes. At each of these places there are indications of a movement from the westward, in accordance with what is shown by the Linton rocks, and also by the striæ on North Berwick Law, as inferred by Mr Stevenson in his paper.

I have also within the last few days had an opportunity of visiting North Berwick Law, and of seeing the rock described by Mr Stevenson. It is the only part of the hill on which I could find smoothing and striation; and it is important to notice that it is the N.W. part of the hill on which these markings occur. I regretted to find, that since Mr Stevenson's inspection and report in the year 1875, most of the smoothed and striated rock has been destroyed by quarrying. But some parts remained; and having in my hand a photograph of the rock, which Mr Stevenson had kindly given to me, I was at no loss to see what had been its principal features. I brought away a specimen of the smoothed surface, which I detached, and on part of which striæ occur. This specimen I now exhibit to the Society.

I found that the smoothed surface generally dipped towards the N. or N.W. at an angle of from 65° to 70°.

Parts of the smoothed surface faced N.W., other parts faced various points towards due N. and even N. by E.; but wherever the rock faced a more easterly point, there was no smoothing. The specimen on the table shows these differences, because it has two faces meeting, forming an angle between them, and fronting in different directions.

The only parts of the smoothed surface striated were those fronting N.W. by N., or a few degrees on either side of that point.

The striæ and ruts were quite as numerous and near one another as on the North British Railway rock.

Their direction was from W. by S. or W.S.W., and most of them were approximately horizontal.

Some of the ruts, especially at their west ends, were deep, showing, as Mr Stevenson says, that the striating agent, whatever it was, must have pressed on the rock with great force. Mr Stevenson also mentions another important fact, which I observed, that *some* of the ruts were inclined along the smoothed face *up towards the east* at angles of from 4° to 20°.

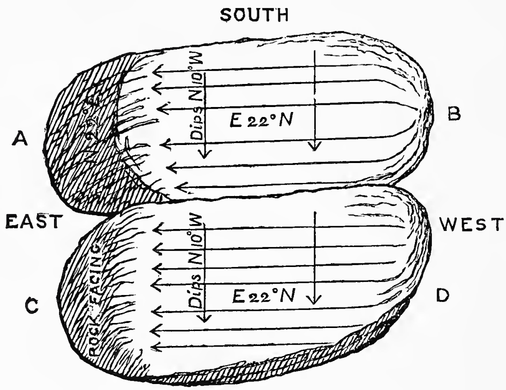


Fig. 2.—Part of Rock, North Berwick Law.

The particular direction in which the striating agent came on the rock from the westward may be inferred, by considering that if it came in a direction *parallel* with the rock it might smooth but would not rut or groove, as there would be no severe pressure. Nor, on the other hand, would it produce grooves or ruts, approximately horizontal and parallel, if it struck the rock *at right angles.* A line parallel with the rock would be S.W., and a line at right angles to it about N.N.W. The intermediate point would be W.N.W., from which direction, therefore, it may be inferred that the striating agent moved upon North Berwick Law.

The annexed, fig. 2, AB and CD, shows a portion of smoothed rock with striæ and ruts. These were only upon the rockface fronting

N. 10° W. At B and D the rock was well smoothed and rounded, and very slightly striated. At the east ends, which faced N. 22° E., there was no striation and little smoothing.

I may add, that some of the rocky ridges in East Lothian at the places above mentioned, present another feature besides smoothing and striation deserving notice, and bearing on this subject.

These ridges, generally speaking, trend or run in a direction E.N.E. and W.S.W. On their N.W. sides, at the base of the ridge, there is often a deep trench, running for some hundred yards, and now filled with water forming little lakes; as, for example, at Smeaton, Balgone, and on the north side of North Berwick Law. The probability is, that the agent which smoothed and striated the *upper* parts of these rocks, had scooped out the softer materials lying along their *base* on their north flanks, forming thereby, as it were, a gigantic ditch or trench on these sides.

II. Striated Rocks in adjoining Counties.

Referring now to striated rocks elsewhere—I would first mention the rock at Stirling Castle, near what is called Drummond's Cemetery,* at a height of from 220 to 230 feet above the sea. The rock

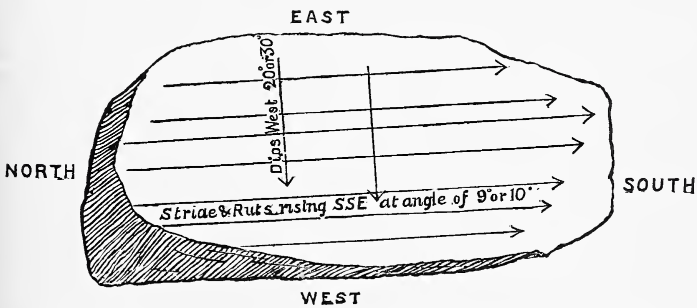


Fig. 3.—Smoothed and Striated Rock, Stirling Castle, 15 × 8 Feet.

there presents a surface which has evidently been smoothed by the friction of some agent which has passed over it. The surface of the rock dips due west at an angle of from 20° to 30°. There are

* The observations recorded in this paper were made by me several years ago. But I regret to find that by the formation of a new walk in the Cemetery, most of the smoothed and striated rock referred to has been removed. A very small portion only remains. This I discovered since the paper was read, and after the proof sheets had come to me for revisal. Happening then to be in Stirling, I went to the Cemetery and found what I have now stated.

numerous striæ on this rock, running about S.S.E., and in that direction rising up along the surface of the rock at an angle of about 9°. Fig. 3 illustrates these facts.

This direction of the striæ upon the surface of the rock, *sloping in the direction it does,* would result from the rock being impinged upon by an agent of great weight and power, moving from W.N.W.

That such was the normal direction of the movement in this district is proved by many markings on other parts of the Stirling Castle rocks. For example, there are several places where there is a narrow defile or gully between the rocks running in a direction approximately W.N.W. In these gullies, though the sides are *smoothed* by the friction of some body or bodies passing through them, they are not *striated,* the pressure on the sides not having been sufficient to produce striæ, these sides having been parallel to the movement of the bodies passing between them.

There are other hills in Stirlingshire which indicate smoothing and striation. Thus, on the Abbot's Craig, near the base of the Wallace Monument, at a height of 334 feet above the sea, there are well-rounded bosses of rock with deep groovings which run N.W. and S.E.

So also at Torwood, about 5 miles to the S.E. of Stirling Castle, as Sir James Hall first pointed out, there are striations on the rock bearing N.W. and S.E., at a height of from 330 to 350 feet above the sea.

Coming now to Mid-Lothian,—on the top of Allermuir Hill, one of the Pentlands, at a height of 1647 feet above the sea, there are striations on the rocks, as vouched by Mr Croll and by Mr John Henderson. On others of these hills, at heights of 900 feet, as vouched by Mr M'Laren, and of 1100 feet, as vouched by Mr Henderson, there are striated rocks. All these are of the same character as those in East Lothian.

Now, with reference to the agent by which these striations may be supposed to have been produced, it is important to keep in view, that at most of the places just mentioned, even at the highest levels, there is abundance of clay and gravel.

Thus, Professor Geikie, in his "Memoir on the Geology of the Neighbourhood of Edinburgh" (p. 126), says: "Boulder clay lies along the N.W. flanks of the Pentlands to the height of at least 1300 feet;" and he adds, that "where the clay has been recently removed, we usually find the rock below polished, grooved, and

scratched, in a direction nearly E. and W., or E.S.E. and W.S.W."

Much to the same effect, Mr John Henderson of this city, an excellent practical geologist, refers to two localities in the Pentlands where clay beds occur full of gravel and hard pebbles. One of these places is Glencorse, at a height of 900 feet, where he says there "is a stiff reddish clay full of well-rubbed and scratched stones, and differing in no way from the boulder-clay of the lower districts." The other locality is 3 miles distant, at a height of about 1100 feet, where (Mr Henderson says) the clay is of the same character as the last-mentioned, and covered by a great deposit of gravel and boulders.

That ruts and striæ on the smooth surface of a rock can be produced by the passage and pressure of hard angular stones, is a fact established by many cases carefully observed. I remember many years ago having witnessed the effects produced by the giving way of a large embankment on the North British Railway at Dunglass Burn. The culvert under the embankment had become choked. Water accumulated on the upper side, till at length the embankment gave way. The materials composing it rushed down the valley with much force; Rocks and large blocks of stone along the valley were scratched and rutted by the *debris* passing over them. I thought the circumstance so instructive that I procured one of the large striated blocks, on which no less than 50 or 60 striæ had been made, and deposited it in the Museum of this Society. I have sought for this specimen, to show it this evening, but without finding it.

III. Information obtained by Study of Boulders.

Whilst considering the agency which produced *striæ* on *rocks,* it is not irrelevant to keep in view the light thrown on the subject by *Boulders.*

Boulders are, like striated rocks, found at all levels, from the sea-shore to the tops of the highest hills. Many of these boulders are traceable to parent rocks situated in the western districts, and therefore show, as the striated rocks do, an agency of great power which moved from the west. For example, the mica slate boulder on the Pentland Hills, 8 or 10 tons in weight, at a height of 1400 feet above the sea, first noticed by Mr M'Laren, must, as he says,

have been carried from about Loch Vennacher or Loch Erne (which is the nearest place for mica slate rocks), distant about 50 miles; and to reach the Pentlands, must have been carried in a S.E. direction across the Ochil range and the valley of the Forth.* A few days ago I was so fortunate as to fall in with a small boulder of red granite, on the farm of Kingston, 2 miles south of North Berwick. This East Lothian granite boulder most probably came also from the Grampians, and travelled 20 miles farther than the Pentland boulders.

There is a large boulder of Carboniferous Sandstone on the Lammermuir Hills, at a height of 1500 feet above the sea, first taken notice of by Professor Geikie in his "Memoir on the Geology of East Lothian," which must in like manner have come from the N.W., where rocks of that description are situated. This boulder led the Professor to say, that it "seemed to indicate a submersion [of the land] to the extent of 1500 feet."

On the farm of Drylaw, near Linton, there is a boulder, with striæ on it, to which my attention was some years ago called by Sir Thomas Hepburn of Smeaton. It was met with on the occasion of a deep drain being made through boulder-clay. The boulder is of basalt, very similar in composition to a rock near the Gullane Hills, situated to the west. The length of the boulder is 6 feet, its width about 3½ feet, and its depth about 3½ feet.

It was narrower at one end than at the other, and that end pointed N.W.

The cutting of the drain having shown striæ on the north side of the boulder near its west end, Sir Thomas Hepburn, on whose land

* With reference to this boulder, Mr Maclaren says:—"To reach the spot where it lies, it must have passed over extensive tracts of country from 500 to 600 feet lower than this spot. Even were all Scotland converted into a *mer de glace*, like Greenland, no moving mass in the shape of a glacier could carry this boulder (and there are many such) from its native seat in Perthshire or Argyleshire to Habbie's Howe. An iceberg from the West or North Highlands, and floating in a sea 1500 or 2000 feet above the present level of the Atlantic, is an agent capable of effecting the transportation of the stone, and offers, I think, the only conceivable solution of the difficulty" (*Edinburgh New Philosophical Journal* for 1846, vol. xl. p. 138). Referring to this boulder, and to another of mica slate on the Pentlands, weighing about ¾ of a ton, the late Professor Nicol says:—"When it is considered that these masses must have been carried upwards of 40 miles in a direct line, floating ice seems the only agent to which their transportation can be ascribed" (*London Geological Society Journal*, vol. v. p. 23).

the boulder lay, had an excavation made in the boulder-clay, along the south side of the boulder, to see if there were striæ on it also. It turned out that there were.

I found that on the north side of the boulder the striæ ran in a direction from W.S.W., and on the south side from N.N.W.

The annexed diagram (fig. 4) represents this boulder, with its north and south sides striated at the west end of the boulder. The striæ were rather more numerous on the north side, AB, than on the south side, CD.

Evidently it was the same agency which produced the striæ on both sides. By coming against the boulder at its west end, this agency, whatever it was, had separated into two streams or *coulées*, and had marked both sides, by pressing upon them as it passed.

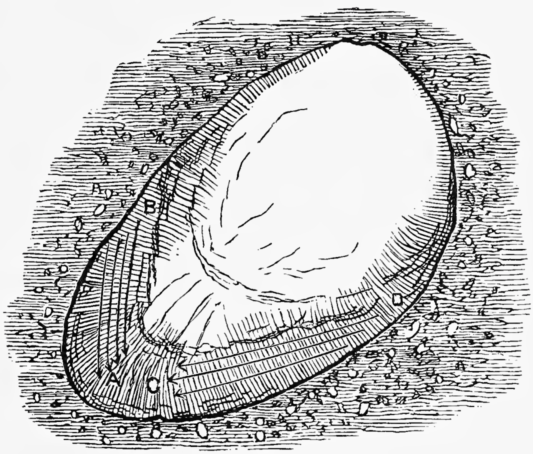


Fig. 4.—Drylaw Boulder, near Linton.

Now this could have been effected only by a body coming from a direction intermediate between N.N.W. and W.S.W., *i.e.*, about W.N.W. The agent which thus divided into two streams must have consisted, not of an inflexible solid body, but of "a mass," as Mr Stevenson calls it, capable of separation. The clay in which the boulder was buried was a body of this character. It contained numbers of pebbles, as usual in boulder-clay, some so hard as with pressure to be capable of smoothing and scratching any rock against which they were pressed. Sir Thomas Hepburn showed to me a portion of a small granite boulder which he had picked out of the clay.

IV. Conclusion.

The facts which I have stated seem to warrant the theory, that when these rocks and boulders were striated, this part of Europe was submerged beneath a sea which reached to the tops of our highest hills, and that ice floated on this sea, carrying boulders and discharging them wherever the ice melted or was arrested by submarine obstructions.

When the sea stood at a high level, effects would be produced on our hill tops and hill sides. As the sea subsided, similar effects would be produced at lower levels.

During the whole period when Great Britain was submerged, we know that the sea was of so low a temperature as to be suited for floating ice. The shells found at a height of 1800 feet in the west of England contain several species of an arctic type. These arctic species occur likewise in Scotland, but at lower levels, when, therefore, probably the sea had greatly subsided.

What all arctic voyagers report as having been seen by them may, therefore, have occurred in Scotland; for they saw rocks smoothed and striated,—and boulders occupying such positions,—as to satisfy them that icebergs, and floating ice in various forms, were the agents which had been, and were then, at work in these phenomena.

With regard to the glacier theory, it seems to me that to account for the striated rocks and boulders *in the valley of the Forth*, that theory is attended with insuperable difficulties. If the striations on North Berwick Law, and in East Lothian generally, were due to a glacier, so must also have been the striations on Stirling Castle rock, the Abbot's Craig, Torwood, and the Pentland Hills. This glacier, therefore, must have been of gigantic dimensions, filling the whole valley of the Forth, reaching to a height of 2000 feet above the present sea-level, and to a depth of at least a hundred feet below it, with a width of some 20 or 25 miles, when at the mouth of the present Firth of Forth. But where could be the birth-place of such a glacier? Certainly not in the valley of the Forth; for the head of the valley is only 220 feet above the sea, that being the height of the ridge which separates the valley of Loch Lomond from the valley of the Forth.

But even if it were possible to suppose that a glacier had been formed at the head of the valley, and that it overspread East Lothian, I find it difficult to understand how rocks, so nearly vertical as those at North Berwick and on the railway could have been striated in the way suggested by Mr Stevenson. Stones or pebbles at the *bottom* of a glacier might, by the weight of the glacier upon them, be made to striate rocks below the glacier, these rocks forming the floor upon which the glacier moved. But rocks which were *vertical*, or nearly so, could not be so operated on; and there are no observations to warrant the supposition that pebbles are ever imbedded in the ice and protruding from the side of a glacier so as to groove the vertical sides of a hill.

The infinite number of the striæ on these steep rocks at Linton and North Berwick Law is also a circumstance most unfavourable to the supposition that they were formed by stones protruding from the side of a glacier. On the other hand, a thick mass of boulder-clay, full of hard pebbles, would be quite capable of striating, if the clay containing them were pushed forward and pressed on a rock surface.

There is another feature which bears on the nature of the striating agent. Mr Stevenson correctly pointed out that whilst most of the ruts and striæ on North Berwick Law were horizontal, some striæ rose upwards towards the east at angles from 4° to 20°. On the railway rock near Linton I observed the same feature. If the striæ were formed by stones protruding from the side of a glacier, they would all be parallel. Their want of parallelism can be more easily explained if a mass of detritus was the agent.

There is one circumstance which appears almost conclusive against a glacier having been the agent of striation, at all events in the valley of the Forth, and strongly favourable to the theory I have indicated in this paper.

This circumstance is the facility with which the striating agent is shown to have been *deflected* from its normal course by trivial obstructions. Thus at Linton, in consequence of the rock on which the striating agent impinged dipping due north, at a considerable angle, that agent, when it came in contact with the rock, was deflected from its E.S.E. normal course to a direction of due south, being a deflection of 22°. In the railway cutting, where the slope of the rock was greater, and therefore more obstructive, the striating

agent was changed from its E.S.E. normal course to a direction of E.N.E., being a deflection of 30° or more. At Stirling Castle rock, in consequence of the rock dipping due west, the striating agent was changed from its E.S.E. course to a direction of due south, being a deflection of 22°. So also by the Drylaw boulder the striating agent was not merely deflected, but made to flow past in two separate streams, each of which differed by several degrees from the normal direction.

Now I feel sure that no glacier, even of moderate dimensions, would have been deflected in its normal course by such obstructions. It would have gone straight over these rock surfaces in conformity with the general movement of the whole body of ice; and certainly when it came against the Drylaw boulder, a block weighing less than two tons, instead of being divided by it into two separate streams, the glacier would have forced the boulder out of its way altogether.

On the other hand, a sea-current would be far more likely to be deflected by such obstructions; and if ice was floating in it of such form and thickness as to reach and plough through the sea-bottom, the mud and gravel there might be pushed forward in such a way as to smooth and striate submarine rocks, whether horizontal or sloping.

But whilst I advocate this theory of floating ice to account for the phenomena in this particular district, I admit that there are some difficulties with which the theory has to contend. For example, if the rock on North Berwick Law, described by Mr Stevenson, was smoothed and striated by a mass of clay and stones pushed and pressed against it, what has become of this detritus? because the rock now stands at least from 20 to 30 feet above the detritus at its base. The same remark may be made as to the striations on the rocks at Stirling Castle and Abbot's Craig, which are still higher above any detritus now on the plains below these rocks. This difficulty, however, vanishes, when regard is had to the enormous denudation in every part of Scotland, of which there is ample evidence. Moreover, in the case of North Berwick Law, there is the remarkable hollow in the detritus along its base on the north side, to which reference has been made, showing that the detritus there has been scooped out to a considerable extent, and this may have happened after the smoothing and striation of the rock.

2. On Methods in Definite Integrals. By Professor Tait.

(Abstract.)

This paper deals with various formulæ of definite integration which are, in general, put into forms in which they enable us with great ease to sum a number of infinite series. As a simple example of such a formula the following may be given :—

$$\int_0^a f'(x)da \int_0^x \frac{\phi'(y)dy}{f(a) - f(y)} = \phi(a) - \phi(0).$$

From this it is easy to deduce innumerable results, of which the annexed are some of the more immediate. They are written just as they are presented by the formula : when different forms are given to f and to ϕ.

$$\int_0^a \frac{dx}{x+1} \log \frac{\log\ (a+1)}{\log \dfrac{a+1}{x+1}} = \log (a+1).$$

$$\frac{1}{1 - \dfrac{1}{a}\dfrac{d}{dp}} \int_0^a \frac{1}{p} (\epsilon^{px} - 1)\, dx = \frac{a}{p}(\epsilon^{pa} - 1).$$

If

$$\frac{1}{\log \dfrac{1}{1-\theta}} = \frac{1}{\theta} + A_0 + A_1\theta + A_2\theta^2 + \ldots\ldots$$

$$\frac{\pi^2}{6} = 1 - A_0 - \frac{A_1}{2}(1 + \tfrac{1}{2}) - \frac{A_2}{3}(1 + \tfrac{1}{2} + \tfrac{1}{3}) - \frac{A_3}{4}(1 + \tfrac{1}{2} + \tfrac{1}{3} + \tfrac{1}{4}) - \&c.$$

$$= \int_0^a \frac{dx}{x} \log. \frac{a-x}{x}.$$

If $1 + \frac{1}{2} + \frac{1}{3} + \ldots + \frac{1}{n-1}$ be put in its lowest terms, the numerator is devisable by n, if n be prime.

The sums of infinite series, such as

$$\sum_0^\infty \frac{\epsilon^{-(x+1)b}}{(x^2a^2+1)((x+1)^2a^2+1)} \text{ and } \sum_0^\infty \frac{(x+1)\epsilon^{-(x+1)b}}{(x^2a^2+1)(\overline{x+1}^2a^2+1)}.$$

Several of the above results are easily verified by the usual methods ; some, however, seem not readily attackable.

Another formula is

$$\int_0^a f(a,x)dx \int_0^x F(x,y)dy = \int_0^a dy \int_y^a f(a,x)F(x,y)dx.$$

3. Measures of certain Radiometer Constants.
By Professor Tait.

4. Further Researches on Minding's Theorem.
By Professor Tait.

5. On the Transmission of Sound by Loose Electrical Contact.
By James Blyth, M.A.

The following Gentlemen were elected Fellows of the Society :—

John Calderwood, F.I.C., Muirhill, West Calder.
John Charles Ogilvie Will, M.D., 12 Union Terrace, Aberdeen.

Monday, 21st July 1879.

DAVID MILNE HOME, LL.D., Vice-President, in the Chair.

MAKDOUGALL-BRISBANE PRIZE.

The Council having awarded the Makdougall-Brisbane Prize, for the biennial period 1876–78, to Professor Geikie, for his Memoir "On the Old Red Sandstone of Western Europe," published in the Society's Transactions 1877–78, which forms one of an important series of contributions by Professor Geikie to the advancement of geological science, the Medal was presented to him by the Chairman, with the following remarks :—

In accordance with what he was told was the practice on such occasions, he would shortly explain, *first*, the purpose for which this particular prize was instituted, and, *second*, the nature of the memoir and work by Professor Geikie for which the prize had been awarded to him.

The purpose of the founder of the prize was to enable the Council, every two years, to mark, by conferring it, its high opinion of any scientific paper or investigation which might come before it.

The Council was of opinion that the memoir by Professor Geikie,

on the Old Red Sandstone formation, which was printed in the last volume of our Transactions, was of sufficient merit to entitle him to the award of the prize, and especially when regard was had to the many other valuable contributions which had been rendered by the author to geological science.

The Old Red Sandstone formation was well known in Scotland, by reason of the many treatises, both popular and scientific, which had been published regarding it, not only by Scotchmen, but by English geologists of reputation, during the last fifty years. But, notwithstanding all that had been done in the way of investigation, the extent of this Formation on the earth's surface was so great, and the variations in its conditions so numerous, that much remained to be done. There was probably no other Formation, known to geologists, which occurred in so many countries, or which presented so many new forms of animal and vegetable life. It occurred in England, Wales, Ireland, Scotland, Norway, Sweden, Russia, India, South America, and Canada. It was so extensively developed in America, that an American geologist, lately writing on the subject, almost made it a matter of national pride, that the formation was more extensive in Canada and the States, than in Europe, and was also richer in fossils. Professor Geikie had intimated his intention of investigating and describing this Formation as it existed, not merely in Scotland, but on the Continent. The memoir lately printed in our Transactions was a commencement of this large undertaking. That memoir was confined to the north of Scotland,—viz., the counties of Moray, Sutherland, Ross, Caithness, Orkney, and Shetland. The boundary of the formation in these parts was indicated by a great belt of shingle, made up from the waste of the Old Silurian rocks, and forming along their northern and eastern flanks a conglomerate rock. The waters which beat on these Silurian rocks, and in which the deposited sediment ultimately became Old Red Sandstone strata, are believed by Professor Geikie to have been lacustrine,—a view originally suggested by the late Dr John Fleming, and adopted by Mr Godwin Austen and other eminent geologists,—judging by the nature of the plants and fish found in the Scotch strata, none of which were considered marine. On this principle, Professor Geikie has applied to the northern district the term Lake Orcadie. The

next memoir, and for which already materials have been largely obtained, will describe the formation as it occurs in the middle district of Scotland, under the title of Lake Caledonia; and the third district, comprehending the south of Scotland and the north of England, will be described under the title of Lake Cheviot.

It was not necessary for him (the Chairman) to refer to Professor Geikie's identification and classification of the rocks composing the strata in the Scotch northern counties, or the different fossils found in them. His object was merely to point out the nature and importance of the investigation entered on by Professor Geikie, and on account of which this prize had been awarded. He had read with the utmost interest the exposition given in the memoir, indicating, as it did, a large amount not only of personal labour on the part of the author, but of great knowledge and ability. He hoped that this prize would be accepted by Professor Geikie as a heartfelt acknowledgment and testimony by the Council of the eminent services he had rendered and is rendering to science by this work. He was sure also that the Professor would be pleased to have his name added to the roll of eminent men of science who had gained this prize in former years, at the head of which roll stands Sir Roderick Murchison, whom Professor Geikie frequently alluded to in his memoir as his "old chief," and whom they all respected not merely for his great geological discoveries, but for the substantial benefits conferred by him on our Edinburgh University, by founding in it that Chair of Geology which Professor Geikie so ably fills.

The Chairman, having then invited Professor Geikie to come forward, placed in his hands the Gold Medal and a bank cheque, adding, as from himself, that it gave him peculiar pleasure to have been called on to perform this duty.

The following Communications were read:—

1. The Solar Spectrum in 1877–8; with some Idea of its Probable Temperature of Origination. By Piazzi Smyth, Astronomer Royal for Scotland.

(*Abstract.*)

This solar spectrum contains measures of about 2000 fixed lines, or fully a third more than are represented in Ångström's worthily

celebrated Normal Solar Spectrum Map, which is taken as the standard reference for "place." The observations were made in Lisbon during the summers of 1877–8, with apparatus prepared by the author; who aimed at including everything visible, from the extreme red to the extreme violet ends of the spectrum, so far as that is amenable to the human eye after transmission through glass.

The author also strove to include only true solar lines, with the least possible admission of such as are produced by the earth's atmosphere. Finally, he compares his so far purified solar result against upwards of 5000 observations of laboratory workers in chemistry, after reducing them to the same spectrum scale; and finds indications that the solar chemical elements are incandescent in a probably far higher temperature than—probably twice as high as—anything yet attained by man.

2. On another Method of Preparing Methylamine. By R. Milner Morrison, D.Sc.

On a former occasion I drew attention to methyl sulphate of ammonium as a suitable material for the preparation of methylamine by heating it with quicklime. There is another and better method of arriving at the same result, the extreme simplicity of which was probably the cause of its being overlooked at the time.

A glance at the formula of methyl sulphate of ammonium shows that it is an isomer of acid sulphate of methylamine, viz. :—

Methyl sulphate of ammonium.	Acid sulphate of methylamine.
CNH_7SO_4.	CNH_7SO_4.
$\begin{matrix} CH_3 \\ NH_4 \end{matrix} SO_4$.	$\begin{matrix} CH_3NH_3 \\ H \end{matrix} SO_4$.

Methyl sulphate of ammonium is an unstable salt, decomposable by a moderate amount of heat, while acid sulphate of methylamine is a much more stable body, hence it becomes probable that if the unstable substance be heated the atoms in the molecules of which it is composed will tend to arrange themselves in that order which is most stable at the temperature to which they are subjected, and therefore that in this case acid sulphate of methylamine will be produced.

Such, in fact, is the case. A portion of *dry* methyl sulphate of ammonium was heated by itself in a sealed tube to about 300° C. for about two hours, nearly the whole of the salt being converted into acid sulphate of methylamine.

When the tube, after cooling, was examined, it was found that a very small portion of the salt had undergone a complete decomposition, some carbon being set free. On opening the tube a small quantity of gas was given off, which was inflammable, and smelt somewhat etherial, with a trace of sulphurous acid. The salt had been fused, and had solidified to a crystalline mass, and now possessed a strongly acid reaction.

This salt, distilled with caustic potash, and the evolved gas collected in hydrochloric acid, the solution, evaporated to dryness on the water-bath, gave a hydrochlorate which, on heating with lime, gave off an inflammable gas in abundance, was deliquescent, soluble in alcohol, and when warm possessed the smell of methylamine hydrochlorate.

Several portions of methyl sulphate of ammonium were heated to different temperatures and for different lengths of time, in order to find out the most advantageous method of preparing the substance.

As there appears to be some regularity in the relation between the temperature and time and the amount converted from one salt into the other, I intend making further research in that direction; and for this reason I have for the present contented myself with only a platinum estimation, for the purpose of determining the amount of methylamine hydrochlorate present in the crude mixture of methylamine hydrochlorate and ammonium chloride. For the purpose of this estimation a portion of the crude hydrochlorate, as obtained by evaporating the hydrochloric acid solution to dryness, was dissolved in water and precipitated with chloride of platinum. The double chloride so obtained was washed, dried, and the platinum estimated by ignition.

The following are the percentages of platina found in the double salts obtained from four experiments under varying circumstances:—

(*a*) 25 grammes of methyl sulphate of ammonium were heated to 300° C. for 2 hours.

(*b*) 25 grammes of the salt were heated to 300° C. for 1 hour.

(*c*) 20 grammes ,, ,, 200° C. for 2 hours.

(*d*) 20 grammes ,, ,, 200° C. for 1 hour.

Per cent. of Pt.

41·6 in methylamine double salt (calculated).
42·0 in double salt from *a* (found).
43·1 " " *b* "
43·43 " " *c* "
43·67 " " *d* "
44·1 in ammonium double salt (calculated).

These numbers represent a percentage of hydrochlorate of methylamine present in the crude salt of—

in (*a*) 84 per cent.
in (*b*) 40 "
in (*c*) 27 "
in (*d*) 17 "

This is also confirmed by the deliquescence of the crude hydrochlorates, which is in exactly the same order and very much greater in the case of (*a*) than the others.

3. On the Composition of "Reh," an Efflorescence on the Soil of certain Districts of India. By J. Gibson, Ph.D.

The following brief statement concerning "Reh" is chiefly derived from the Report of the Committee on Reh, November 1878:—

Large tracts of country in the north-west of India have from time immemorial been covered with a white alkaline efflorescence, which renders them incapable of supporting any form of vegetable life. These tracts are called "usar"* plains, and the white efflorescence "reh."† The appearance of this reh on the surface is owing to the subsoil water, which is impregnated with sodium salts, being sucked up to the surface by capillary attraction and there evaporated by the fierce heat of the Indian summer sun. The reh is often very irregularly distributed,—bald patches occurring in the midst of cultivation, or cultivated patches surrounded by reh,—and this capricious distribution has rendered a right comprehension of its

* From the Sanscrit, signifying "barren land."
† Hindee word for saltpetre.

mode of production difficult. It seems certain, however, that its appearance depends chiefly on the distance of the water-table from the surface of the ground and upon the nature of the intervening soil.

When reh exists in the soil it is more largely developed on a surface where the water level is nearer the surface; while, on the other hand, a soil of a close texture,—a clay soil, for instance,—may prevent the formation of reh by hindering the upward passage of the water, even where the water-table is comparatively near the surface. Of course anything which affects the rapidity of evaporation must affect the rapidity with which the reh accumulates, so that the cutting down of trees, and thus exposing the surface of the soil to the unmitigated action of the sun's rays, has an injurious effect and tends to promote the formation of the efflorescence. Until lately an equilibrium of reh distribution was established, at all events approximately. The great usar plains which have existed from unknown times did not increase in extent or change their position; but it would seem that by the introduction of the canal-irrigation system this equilibrium has been disturbed. The Western Jumna Canal—which runs from the Jumna, where it leaves the hills, down as far as Delhi—has developed large tracts of reh, land which was formerly cultivated and fruitful being covered with it, and the same process has begun and is going on with alarming rapidity on the Ganges and Eastern Jumna canals in the country between the Jumna and the Ganges. The evil is of great magnitude, and large tracts of fertile land are fast becoming waste and unproductive, and the condition of the people and their cattle is deteriorating in consequence. Two years ago a committee was appointed by Government to discover the cause or causes of the evil, and suggest, if possible, some remedy. As the result of their deliberation, it is established beyond doubt that this new production of reh is chiefly, if not solely, caused by the system of canal irrigation as at present carried out. In the first instance, the level of the salt-impregnated subsoil water has been much raised, so as to be more readily drawn up to the surface by capillary action and there evaporated, leaving the salts it held in solution as a crust on the surface. There seems to be some difference of opinion amongst the members of the committee, as to whether this is chiefly due to percolation from the bottom and sides of the canals, which are at a

higher level than the surrounding land, or to the system of flush irrigation, which is often so wastefully carried out as to swamp the lands watered. Moreover, in many cases the canals and their distributaries have been so carelessly aligned as to interfere seriously with the surface drainage. The canal water, itself containing more or less of these sodium salts in solution, is a new and inexhaustible source of reh, and though the process in this case may be slow, it is sure, ultimately, to result in the appearance of reh and the consequent unproductiveness of the land.

Various remedies for this state of things have been suggested, some of them very desperate, such as an entire remodelling of the canals, or an extensive system of deep drainage, or the substitution of what is called "lift" for "flush" irrigation, which would mean that every drop of water used be lifted from reservoirs and distributed over the land by manual labour. All efforts to reclaim land once infected with reh, by cultivation or otherwise, have proved abortive.

Mr J. Wilson, M.A. Edin., Assistant Settlement Officer, having sent a small sample of reh to Professor Wilson, with the request that it be analysed, the investigation was kindly intrusted to me, and I now beg to lay before the Society the results of my analysis.

Results of Analysis of Sample of Reh.

	Moisture	= 2·5
	Insoluble inorganic residue*	= 68·1
Soluble in Water.	Na	= 9·5
	Fe	= 0·1
	NH_3	= 0·35
	SiO_2	= 0·35
	SO_4	= 12·1
	CO_2	= 3·6
	Cl	= 1·9
	Organic matter	= 0·7
	Mg	= trace
		99·2

* Organic matter insoluble in water was not estimated.

Percentage Composition of Soluble Portion.

Na	= 32·4
Fe	= 0·5
NH_3	= 1·2
SiO_2	= 1·2
SO_4	= 41·3
CO_2	= 12·3
Cl	= 8·8
Organic matter	= 2·4
Mg	= trace
	100·1

From these results it will be seen that the portion of this sample soluble in water, which I suppose to be the reh proper, is mainly a mixture of sulphate of soda, carbonate of soda, and common salt, the sulphate being much the most abundant. I hope soon to have an opportunity of analysing some more samples of reh, the results of which analyses I shall communicate to the Society.

4. On Spherical Harmonics. By Professor Tait.

5. Proposed Theory of the Progressive Movement of Barometric Depressions. By Robert Tennent, F.M.S.

This paper is a continuation and a fuller explanation of the previous one, in which it was attempted to show why barometric depressions, or storms, move forward. In our present state of knowledge, exceptional cases in such an investigation are always to be found, and the subject is consequently taken up in a general point of view. It is attempted here to be shown, not how depressions always move, but how, under certain well-known circumstances, they *may* move.

A depression of a small diameter was shown to be found under a comparatively high atmosphere, in which case it would tend to fill up. But when its diameter is great, say of several hundred miles, and underneath the same real atmospheric height, it may then be considered as existing under a comparatively low atmosphere, which

may be represented by the thickness of a sheet of paper spread over an ordinary globe. The difference in the mechanical effects which must then take place are evidently of great importance. In this latter case, the effect of the earth's rotation will be fully introduced, and the inflow of the winds to the low barometric centre will here have to pass over a great extent of resisting surface, the effect of which was shown. Under these circumstances, a depression will not, as in the former case, tend to fill up; it will now tend to open out, and in one particular direction, which is that of progressive movement. It does not move as a rigid aerial cavity forced onwards by high pressure in the rear, as it is by many supposed to do, but not being a rigid aerial cavity, it can only advance, as it sometimes does, at 70 miles an hour, *as a circular atmospheric wave*, by opening out in front, which may be regarded as being lateral and horizontal extension, and by filling up in the rear. It is difficult to conceive how progress can take place in any other way. This mode of advance was shown to take place only on a resisting surface, and not on a frictionless surface, on which depressions could not advance at all; although the universally received opinion is, that a resisting surface retards progressive movement instead of facilitating it. Storms of the usual diameter move in *direct contact* with the surface of the earth, and not over a mass of calm air resting upon it. Query, Can they move through space? Conclusions were arrived at in a former paper on the barometer, that *its rise and fall was greatly due to the passage of the air over a resisting surface;* on a frictionless surface this would be greatly diminished, and consequently much less difference would then be found to exist betwixt high and low-pressure, which causes and accompanies storms; but if this is not to be found, it will then show one of the reasons why they do not move over a frictionless surface, on which they cannot be opened out by the rotation of the earth.

The motive force which causes storms to move, is here supposed to be a central ascending current which carries off the spiral inflowing winds, which in the different segments are not uniform, either as to their direction or their velocity. Movement takes place in the direction in which supply is least copious, where the isobars are widest, and where inflow is most direct, as shown by Rev. W. Clement Ley in the *Scottish Meteorological Magazine*, with, of

course, as above stated, exceptional cases, which may be afterwards explained. This is shown in the diagram of the ascending balloon.

The rise and fall of the thermometer is equal both above and below its mean, but the *extent of the rise and fall of the barometer is much greater below than above its mean.* This was accounted for so far by "lifting" or fictitious pressure, which accompanies southerly winds, which prevail when the barometer is below its mean.

The second part of this paper is intended to show why storms require high pressure to the right of the direction in which they advance. This is represented by a diagram of a flat wheel, which is laid horizontally on the surface of the ground; it circulates round its centre, which also moves forward in one direction. It is in this way that a depression also moves, say, in an easterly direction. Its winds have a circular rotatory movement round its low centre in a direction opposite to that of the hands of a watch. Let their circular rate of speed be at 30 miles per hour, and let the forward movement of the low centre also take place at the same rate of speed. The consequence of this will be that east winds on the north segment will be at rest—calm—and not move over the ground; at the same time they must necessarily form a segment of the circular rotation round the low centre. If thus at rest, they must cause a collapse, and a termination of the circular rotation of the depression. But they do not do so. To account for this, these east winds may be regarded as being *space* winds, and as such in this way their circular rotation may be maintained. As space winds they actually move, though not over the surface by which their speed is estimated, and hence they may be *said to blow when they do not blow.* West winds on the south segment, having their velocity regarded as being made comparable with the mode in which east winds blow, when they are supposed to be at rest, may be said *to blow more rapidly than they really do blow*, because their speed is not merely to be represented by their circular rotation round the low centre, but also by the additional speed which they acquire in the direction of the advancing low centre. Under these circumstances, *the velocity of east and west winds are not comparable as estimated by their passage over the surface.* To render them comparatively equal, they must be regarded as being both space winds and earth winds.

East winds consequently pass over a less extent of surface when they are not at rest, because they move in a direction opposite to that of progress, and consequently do not require high pressure to aid them on their segment. West winds, which have greater velocity, and pass over a much greater extent of surface, and in the direction of progress, require high pressure on their segment. It is in this way that the point is accounted for. Storms do not move against high pressure in front, except in those cases in which it is far distant.

6. On the Solution of the Simultaneous Equations:— $ax+by=c$ and $dx+ey=f$, when the Symbols denote Qualities. By Alexander Macfarlane, D.Sc.

Donations to the Library of the Royal Society during Session 1878–79.

I. Transactions and Proceedings of Learned Societies, Academies, etc.

Adelaide, South Australia.—Adelaide University Calendar for 1879.

Catalogue of the Plants under Cultivation in the Government Botanic Garden, Adelaide. R. Schomburgk, director. 8vo. 1878.

South Australian Industries and Manufactures. 8vo. Adelaide, 1875.

South Australia: Its History, Resources, and Productions. Edited by W. Harcus. 8vo. Adelaide, 1876.

Progress and Condition of the Botanic Garden and Government Plantations. 1878.—*From the University of Adelaide.*

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Amsterdam.—Verhandelingen der Kon. Akademie van Wetenschappen. Afd. Natuurkunde, Deel XVIII. Verslagen en Mededeelingen, Natuurk. 2e Rks., Dl. XII., XIII. Letterk. 2e Rks., Dl. VI., VII., 1877, 1878. Processen Verbaal, 1876–77, 1877–78. Jaarboeken, 1876, 1877. Idyllia, aliaque Poemata, 1878.—*From the Society.*

Flora Batava: Afbeelding en Beschrijving van Nederlandsche Gewassen. Voortgezet door F. W. Van Eeden. 241–244 Afleveringen.—*From the King of Holland.*

Basel.—Verhandlungen der Naturforschenden Gesellschaft. Sechster Theil, Viertes Heft. 1878.—*From the Society.*

Batavia.—Observations made at the Magnetical and Meteorological Observatory. Vols. II. and III. 1869–75. 4to.—*From the Observatory.*

Berlin.—Abhandlungen der Königl. Akademie der Wissenschaften, 1877 and 1878.

Monatsberichte der Kön. Preussischen Akademie der Wissenschaften. Jun. 1878–Jun. 1879.—*From the Academy.*

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PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. X. 1879–80. No. 105.

Ninety-Seventh Session.

GENERAL STATUTORY MEETING.

Monday, 24th November 1879.

DAVID STEVENSON, Esq., M.I.C.E., in the Chair.

The following Council were elected:—

President.

The Right Hon. Lord MONCREIFF.

Vice-Presidents.

The Right Rev. Bishop Cotterill.
Principal Sir Alex. Grant, Bart.
David Milne Home, LL.D.
Sir C. Wyville Thomson, LL.D.
Prof. Douglas Maclagan, M.D.
Prof. H. C. Fleeming Jenkin, F.R.S

General Secretary—Professor Tait.

Secretaries to Ordinary Meetings.

Professor Turner.
Professor Crum Brown.

Treasurer—David Smith, Esq.

Curator of Library and Museum—Alexander Buchan, M.A.

Councillors.

Professor Rutherford.
Dr R. M. Ferguson.
Rev. W. Lindsay Alexander, D.D.
Dr Thomas A. G. Balfour.
J. Y. Buchanan, M.A.
Rev. Thomas Brown.
Robert Gray, Esq.
Dr William Robertson.
Professor Campbell Fraser.
Professor Geikie.
Rev. Dr Cazenove.
David Stevenson, Mem. Inst. C.E.

The following are *ex officio* Vice-Presidents, having filled the office of President:—

His Grace the DUKE of ARGYLL, K.T., D.C.L.
Sir ROBERT CHRISTISON, Bart., M.D., D.C.L.
Sir WILLIAM THOMSON, Knt., LL.D.

Monday, 1st December 1879.

The Right Hon. Lord Moncreiff of Tulliebole, President of the Society, occupied the Chair, and delivered the following Opening Address:—

Gentlemen,—

I cannot, of course, commence the discharge of the duties of President of this learned and celebrated Society, without expressing my profound gratitude to the members for their unexpected kindness in elevating me to this chair, and my intense appreciation of the distinction they have thereby conferred upon me. It is a position of which any man, however high his reputation or however great his attainments, could not fail to be proud. To me your favour has laid me under the greater obligation, that I am painfully conscious that I am entirely without pretensions to the special qualities which have generally determined your choice. I am but a loiterer outside the temple of science, waiting for what it may please her priests to dispense to the crowd. My incursions into the fields of literature have been so rare, so desultory, and so clandestine, that I own myself more of a trespasser than one entitled to be there. Nevertheless, as fortunately for me your stamp has been impressed, it is not for me to gainsay so honourable and gratifying a dignity. The coin must pass current, whatever may be the intrinsic value of the metal. It may be, perhaps, that in the course of a life, not now to be reckoned short, spent in public and professional labours with various surroundings, I may, while little space has been left for cultivating the proper attributes of this chair, have acquired some knowledge of detail, some acquaintance with habits and springs of thought and action, which may be brought usefully to bear on the discharge of my duties. Be that as it may, I can only assure you that as far as my ability may reach, or industry and attention can serve me, I shall do my best not to discredit the appointment you have made.

For the rest, as regards the discharge of my duty to-night, I must ask you to make allowance for the suddenness with which it has overtaken me, and for the numberless distractions which have abridged even the short time I have had for preparation. I regret

this the more that there are some topics on which I should like to have enlarged, and which might have been appropriate to the occasion, and not without interest. I hope to have an opportunity hereafter of fulfilling more perfectly a duty which to-night must remain to a large extent unperformed. I understand that my being selected for this office to a certain degree is due to a desire on the part of the Society to vary for once the special qualifications which have hitherto been mainly looked for in your President, and to indicate a desire to revert to the literary side of the Society, as was contemplated in its original constitution. As far as the absence of any pretensions as a physicist is concerned, I no doubt may be a fitting representative of the desire so indicated, although the transition from the negative to the positive is much more doubtful. But I am to a certain extent consoled, in the sense of my own deficiencies, by finding that a professional position as a lawyer was once considered not without its recommendations in the choice of your members, or even in the selection of your President; for I find that the first meeting of the Society was held on the 23d day of June 1783, when the Right Hon. Thomas Miller of Barskimming, Lord Justice-Clerk, was chosen President of the meeting, and it was resolved that the Lords of Council and Session, and the Barons of Exchequer in Scotland, should be invited to a participation of the Society's labour, an invitation which, I am glad to think, was largely responded to at the time, and might with mutual advantage be more generally acted on now.

The theme which I should have chosen for this address, had I been able to cast it in anything of a systematic mould, would have been to consider how far it might be possible or expedient to widen or strengthen the literary character of this institution, and if so, in what direction, and towards what result this might be attempted. Times are greatly changed since the Society was formed, nearly a century ago. The prosecution of physical science, and the recording of its progress from month to month, must always be its chief development; for physical science is necessarily progressive, and every step taken is a step in advance. But with literature, or mental science, or political or social economy, or dialectics, it is not so; and if the meeting and if this Society were to exchange the weighty authority of its scientific transactions for the ephemeral

smartness of a literary institute, it would lose both in intrinsic interest and importance, and in public estimation. Still I think there is room for some good work to be done in relation to Scottish literature, being cognate to the grave and solid character which is and has always belonged to the transactions of the Society; and it had occurred to me, in searching for a suitable theme for my address from this chair, that some interest might be created by tracing in detail the rise and progress of English composition in Scotland. It is a subject which has never been thoroughly investigated or exhausted. We know more of Scottish literature in the sixteenth century than we do of that of the seventeenth. The period I should wish particularly to have illustrated is that long, and, as regards our own literature, rather dreary and barren chasm between the union of the crowns in 1603 and the union of the kingdoms in 1707. I should like to trace, if it could be done, the gradual steps by which our Scottish writers ascended from the strong, nervous, but Scottish vernacular of the 15th century, until, in the persons of Hume, Robertson, and Adam Smith—the founders of this Society—or among them, they became masters and models of the art, and gave laws, and canons of criticism, to the authors south of the Tweed. I have always looked on this result as a very remarkable instance of the tenacity and adaptability of the Scottish character. There can be no doubt that as regarded the arts and sciences, the more refined pursuits of the intellect, and the inducements to cultivate the gentler tenderness of knowledge, the union of the crowns in the first instance, and the union of the kingdoms ultimately, were a deep and severe blow. They withdrew money and patronage from Scottish enterprise in the markets of intellect as well as in those of commerce; and I incline to think that this cause, more than the troubles of the times, explains the lack of Scottish literature in the first half of the seventeenth century. The union of the kingdoms was a second and sudden discouragement to Scottish literature. Left without either court or parliament, and forced to conform to English models, authorship in Scotland became a very dreary affair. Scottish language, accent, phrases, and speech were the subject of perpetual derision to their southern neighbours, and the old Scottish pride of her authors could ill brook, while they could not withstand, the current of contemptuous criticism which assailed them. David

Hume speaks with great bitterness of the stings and "arrows" which galled him so much in London, compared with the deference and adulation which followed him in Paris. It has been said that the true secret of the leaning towards arbitrary power evinced by his History, especially in its later revisals, was inspired by the fact that James the First of England spoke broad Scotch, and that the English were unpleasantly proud of their free constitution. But the uses of adversity proved salutary. The Scotsmen had their revenge; for whatever may be thought of Hume's politics, his style remains to this day without a rival, and the same brotherhood of strong men who founded this Society, achieved a triumph in the fields of literature which remains a lasting monument of their power.

Such was the colour of my thoughts when I first became aware that I should have to-night to address you for the first time from this chair, and such were the lines on which, had my time permitted, I should have endeavoured to illustrate the literary side of your escutcheon. But the mere sketch which I have now outlined must I fear satisfy you that the task could not be performed with anything like success, without an amount of honest research to which my time has proved entirely inadequate.

STATISTICAL STATEMENT.

The following statement, in regard to the present Fellows of the Society, has been drawn up by the Secretary:—

1. Honorary Fellows:—

Royal Personage—

His Royal Highness the Prince of Wales, 1

British Subjects—

John Couch Adams, Cambridge; Sir George Biddell Airy, Greenwich; Thomas Andrews, M.D., Belfast (Queen's College); Thomas Carlyle, London; Arthur Cayley, Cambridge; Charles Darwin, Down, Bromley, Kent; John Anthony Froude, London; Thomas Henry Huxley, London; James Prescott Joule, Cliffpoint, Higher Broughton, Manchester; William Lassell, Maidenhead; Rev. Dr Humphrey Lloyd, Dublin; William Hallowes Miller, Cambridge; Richard Owen, London; Thomas Romney Robinson, D.D., Armagh; General Sir Edward Sabine, R.A., London; Henry John Stephen Smith,

Carry forward, —— 1

Brought forward, 1

Oxford; Professor Balfour Stewart, Manchester; George Gabriel Stokes, Cambridge; James Joseph Sylvester, Baltimore; Alfred Tennyson, Freshwater, Isle of Wight, ... 20

Foreign—

Robert Wilhelm Bunsen, Heidelberg; Michel Eugène Chevreul, Paris; James D. Dana, LL.D., New Haven, Connecticut; Alphonse de Candolle, Geneva; Franz Cornelius Donders, Utrecht; Jean Baptiste Dumas, Paris; Carl Gegenbaur, Heidelberg; Asa Gray, Cambridge, U.S.; Hermann Helmholtz, Berlin; Jules Janssen, Paris; August Kekulé, Bonn; Gustav Robert Kirchoff, Berlin; Hermann Kolbe, Leipzig; Albert Kölliker, Würzburg; Ernst Eduard Kummer, Berlin; Richard Lepsius, Berlin; Ferdinand de Lesseps, Paris; Rudolph Leuckart, Leipzig; Johann Benedict Listing, Göttingen; Joseph Liouville, Paris; Carl Ludwig, Leipzig; J. N. Madvig, Copenhagen; Henri Milne-Edwards, Paris; Theodor Mommsen, Berlin; Louis Pasteur, Paris; Professor Benjamin Peirce, U.S. Survey; Karl Theodor von Siebold, Munich; Otto Struve, Pulkowa, St Petersburg; Bernard Studer, Berne; Otto Torell, Lund; Rudolph Virchow, Berlin; Wilhelm Eduard Weber, Göttingen; Friedrich Wöhler, Göttingen, ... 33

Total number of Honorary Fellows at November 1879, ... —— 54

The following are the Honorary Fellows deceased during the year:—

Foreign.—Heinrich Wilhelm Dove, Johann von Lamont. M. Charles Dupin died in 1873.

2. Ordinary Fellows:—

Ordinary Fellows at November 1878, ... 385

New Fellows, 1878–79.—James Abernethy, Esq.; James Lambert Bailey, Esq.; Dr George William Balfour; James Blaikie, Esq.; John Calderwood, Esq.; Robert Cox, Esq., of Gorgie; William Denny, Esq.; Professor Cossar Ewart; Thomas Gilray, Esq.; John Hislop, Esq.; T. H. Cockburn Hood, Esq.; Dr Alexander Bennett M'Gregor; Dr Francis W. Moinet; J. B. Brown Morrison, Esq., of Murie; Major-General A. Cunningham Robertson; John

403

	Brought forward,	385
Turnbull, Esq.; Dr John Charles Ogilvie Will; Dr Andrew Wilson, ...		18
Deduct Deceased.—Alexander J. Adie, Esq.; John Blackwood, Esq.; Dr Colledge; E. W. Dallas, Esq.; Dr J. G. Fleming; Edward J. Jackson, Esq.; Professor Kelland; Dr James M'Bain; Professor Clerk-Maxwell; Professor Nicol; Dr Montgomerie Robertson; J. F. Rodger, Esq.; Dr John Smith; Sir Walter C. Trevelyan; Arthur, Marquis of Tweeddale, ...	15	
Resigned.—Professor Fuller; O. G. Miller, Esq.; Professor John Young, ...	3	
		18
Total number of Ordinary Fellows at November 1879, .		385
Add Honorary Fellows, ...		54
Total number of Ordinary and Honorary Fellows at commencement of Session 1879–80, ...		439

During the last session the Keith Prize for the biennial period 1875–77 was awarded to Professor Heddle for his papers on the "Rhombohedral Carbonates" and on the "Felspars of Scotland," originally communicated to the Society, and containing important discoveries. The Makdougall-Brisbane Prize for the biennial period 1876–78 was awarded to Professor Geikie for his memoir "On the Old Red Sandstone of Western Europe," which has been published in the Society's Transactions, and forms one of an important series of contributions by Professor Geikie to the advancement of geological science.

OBITUARY NOTICES.

The Rev. Professor Kelland. By Professors Chrystal and Tait.

Professor Kelland was the son of the Rev. Philip Kelland, who, at the time of the birth of his son, was rector of the parish of Dunster, in Somersetshire. Afterwards it would appear that he removed to Landcross, in Devonshire. Though an Oxford man himself, he sent his son Philip to Queen's College, Cambridge, where he greatly distinguished himself among his contemporaries, and in 1834 stood at the head of the honour list as Senior Wrangler and First Smith's Prizeman. Mr Kelland, who had taken orders in the Church of England, became Tutor of Queen's College, and held the post for

the next three years. In 1838 he was appointed to the Chair of Mathematics in the University of Edinburgh, as successor to Professor Wallace. He has been a Fellow of this Society ever since he came to Edinburgh; and only last year was elected to its highest office.

The loss which this Society suffered in the death of its President has already been characterised in fitting words by Sir Alexander Grant (*ante* p. 208). What we are now called upon to do, is to give a general account of his services alike to the University of Edinburgh and to science.

Kelland occupied the Mathematical Chair from the time of the resignation of Professor Wallace in 1838—a period including forty-one complete sessions. During six months of each year he gave at least thirteen lectures in all per week to his three classes; and for at least four sessions, during the illness of Professor Forbes, he conducted the Natural Philosophy course also. He was, besides, for long periods secretary of Senatus, and of the Board of Visitors of the Observatory, and was constantly employed in conducting examinations for various public bodies and institutions, *e.g.*, the Colleges of Physicians and Surgeons, the Dick Bequest, the Edinburgh High School, &c., and on several important occasions his services were engaged by one of the Scottish Insurance Offices with a view to the septennial investigation of its affairs from the actuarial point of view. In this connection he made a tour in Canada and the United States, of which he published an account in a charming little volume called "Transatlantic Sketches." If we add to this the labour entailed by his various published works and original scientific papers, as well as his constant contributions to educational publications, we can easily see what an active life he spent. To the very end of his career his activity never seemed to flag. His college duties grew from year to year, partly in consequence of the great increase in the number of students, but mainly because of the enormous increase of graduation. And he kept up, year after year, from 1869 at least, two Mathematical Classes for the Edinburgh Ladies' Educational Association. Yet his teaching was to the last as thorough as ever; and no better proof could be desired than the fact that three of the last four awards of the Ferguson Mathematical Scholarship, which is open to all the Scottish Universities, have been made in favour of Edinburgh students.

As Sir A. Grant has well said, he came to know the Scottish Universities better even than do Scotsmen themselves. To this we may add that he knew also, as few have ever known them, the characteristics and the wants of Scottish students. Our grief for his loss is at least tempered by the fact that he died at his post after an unusually extended term of active usefulness. He who has in person instructed, alike by clear precept and noble example, many thousands of the youth of a nation, cannot fail to have a happy and lasting influence on that nation's progress. Philip Kelland was, in the very highest sense, a benefactor to Scotland.

In spite of all his hard week-day work, he did not shrink from clerical duty on Sundays, very often reading the service, or preaching, in some of the Episcopal Churches in Edinburgh. In his sermons, as in his secular addresses, he was studiously quiet and simple, avoiding all mere popular arts of word-painting; but he was none the less effective in consequence. No one in the crowded Assembly Hall on the 22d April last, when he appeared for the last time before the public, can forget the profound impression produced on the whole audience by the few but earnest and loving words which he then addressed to the graduates. A fortnight later he was followed to his grave by the majority of those who had vociferously applauded his simple and touching eloquence.

His earlier mathematical work was very much influenced by his admiration for Fourier and Cauchy. The latter, indeed, was his personal friend. His *Theory of Heat*, and various elaborate papers in the "Camb. Phil. Trans." and the "Phil. Mag.," show how Kelland attempted to base the explanation of the phenomena of heat upon the mutual action of systems of particles exerting forces on one another at a distance. The analysis employed is of a nature very similar to Cauchy's; but we need not examine these attempts closely, for, though they show great mathematical ingenuity, they are now known to be based upon an unsound physical assumption.

He was much more successful when his physical assumptions were more accurate, as in his investigations of the motion of waves in canals, and in the calculation of the intensity of light which had passed through a grating. Another real service which he did to physical science consisted in his having edited and reprinted the

very valuable "Lectures" of Thomas Young. Kelland's edition is now unfortunately, like its predecessor, entirely out of print.

But his forte unquestionably lay in pure mathematics, and in that department, and others closely allied to it, he has enriched our "Transactions" with several very excellent papers.

An idea of Professor Kelland's scientific activity will be obtained by looking at the list of papers under his name in the Royal Society's Catalogue of Scientific Memoirs.

Several of his memoirs deal with physical optics. Two of these are especially interesting. They deal with the question of the aggregate effect of interference. In the first ("Trans. Camb. Phil. Soc." vii.) he shows that when light falls on a lens, part of which is covered and part uncovered, the whole quantity of light on a screen placed in the focus is to that which falls on the lens as the area of the uncovered part of the glass is to the whole area of the glass. Hence he infers that the whole quantity of light is not diminished or increased by interference. In the second ("Trans. R.S.E." xv.), starting from the principle thus established, he treats a very interesting point which arises in the application of Huyghens' principle in the undulatory theory. In forming the expression for the vibration due to any element of an aperture on the surface of a lens, we multiply the maximum intensity of vibration by the area of the element, and to keep the dimension correct we must divide by a factor D whose dimension is the square of a line. Kelland investigates a variety of cases for different forms of aperture, and finds in each case that D must be $b\lambda$ where λ is the wave-length of the incident light, and b the distance from the lens of the screen placed in its focus. The question was afterwards discussed by Stokes ("Trans. R.S.E." xx.), who generalised Kelland's analysis, and showed that the result may be deduced for an aperture of any form.

In a memoir read before the Royal Society of Edinburgh in April 1839 ("Trans." xiv.) Kelland took up the subject of wave motion. He discusses the case of a canal of finite depth h, adopting the hypothesis of parallel sections. Assuming the motion to be undulatory, and taking

$$z = h + a \sin \frac{2\pi}{\lambda}(ct - x)$$

for the equation to the surface, he deduces the approximate formulæ, in which $a = \frac{2\pi}{\lambda}$,

$$c^2 = \frac{g}{a}\frac{e^{ah} - e^{-ah}}{e^{ah} \quad {}^{-ah}} \cdot \frac{1}{1 - a^2\alpha^2(e^{ah} - e^{-ah})} \quad . \quad . \quad . \quad . \quad . \quad . \qquad (1)$$

$$a = \frac{2}{\alpha(e^{ah} + e^{-ah})} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2)$$

the first of which gives the velocity of transmission, the second the height of the wave.

In the latter part of the paper he applies his method to canals having a vertical section of any shape whatever, and deduces the following elegant formula—

$$c^2 = g\,\frac{\text{area of vertical section}}{\text{breadth at surface}}$$

for the velocity of propagation. This gives the result, for canals of triangular section, that the velocity of propagation is that in a rectangular canal of half the depth. This conclusion is tested by means of Scott-Russell's observations, and is found to be in close agreement with fact.

The same result was also arrived at independently by Green, who, in point of fact, anticipated Kelland in the matter, for he gives it in a note read before the Cambridge Philosophical Society on the 18th February 1839, whereas Kelland's paper was read on the 1st April of the same year. Scott-Russell's observations were the exciting cause of both investigations, which have little in common beyond this particular result.

In a memoir on General Differentiation ("Trans. R.S.E." xiv.), read December 1839, Professor Kelland deals with one of the most abstruse and difficult branches of analysis. The process by which we extend the meaning of the symbol x^m_ι where m is integral to the case where m has any value whatever, is familiar enough, although it has its difficulties as every algebraist knows. General differentiation is a problem of a similar kind but of a much higher order of difficulty. Thus,

$$\frac{d}{dx}, \qquad \left(\frac{d}{dx}\right)^2, \qquad \left(\frac{d}{dx}\right)^3, \text{ \&c.,}$$

are symbols which have for their effect to deduce in a particular

way from any given function another set of functions, to which the name of first, second, third, &c., differential co-efficients are givèn. The corresponding problem here is to interpret the operating symbol,

$$\left(\frac{d}{dx}\right)^\mu,$$

where μ may have any value whatever. This interpretation must be so made that it shall include the particular meanings already attached to the cases where μ is integral. This process of extension has been aptly called by De Morgan a case of the interpolation of forms, and there are difficulties in connection with it very like those that arise in the solution of functional equations or the inverse method of finite differences. The question was first raised by Leibnitz, and was treated successively by Euler, Laplace, Fourier, Liouville, Greatheed, Peacock, and Kelland.

The laws of operation to be conserved are—

$$D^n(u+v) = D^n u + D^n v,$$
$$D^m D^n u = D^{m+n} u.$$

The only question is:—What fundamental functions are we to select on which to base our calculus? It appears that different systems arise according as we select our fundamental functions. Peacock starts with x^m: Kelland, following Liouville and others, starts with e^{mx} as the ground function, and lays down the equation

$$\left(\frac{d}{dx}\right)^\mu e^{mx} = m^\mu e^{mx}$$

as the foundation of his system.

By means of a definite integral he then deduces the general formula

$$\left(\frac{d}{dx}\right)^\mu x^{-n} = \frac{(-1)^n \lfloor\!\overline{n+\mu}}{\lfloor\!\overline{n}\, x^{n+\mu}},$$

where $\lfloor\!\overline{n}$ is a function like the gamma function, satisfying the equation

$$\lfloor\!\overline{n+1} = n \lfloor\!\overline{n},$$

but unlike it not restricted to positive values of n.

This formula is, then, applied in a variety of particular cases, and is shown to be perfectly general provided certain conventions are adopted, and from it are derived working formulæ convenient in different cases.

The theory is applied to the logarithmic and circular functions, and at the end of Part I. are given some very ingenious applications to expansions in fractional powers of x.

In Part II. is given the following extremely elegant formula—

$$\int_0^z d\theta\phi(\theta+a)(z-\theta)^p = (-1)^{p+1}\sqrt{p+1}\cos(p+1)\pi\left(\frac{d}{dz}\right)^{-(p+1)}\phi(z+a),$$

which is applied to the solution of a variety of problems.

The whole of the mathematical work in this memoir is of great simplicity and elegance, and for that reason alone it is well worth the attention of students of the higher mathematics. It has, moreover, intrinsic value as an important contribution to the elucidation of a difficult branch of analysis. How great that importance may be it is impossible to estimate until the future of the method is more certain than it can at present be said to be; but, in any case, the work will remain a lasting monument to the skill and ingenuity of its author.

Closely connected with the paper just mentioned is another on a process in the differential calculus and its application to the solution of differential equations. Nothing farther need be said regarding it except that it is characterized by the same elegance and simplicity that mark the memoir on general differentiation.

Perhaps the most important of all Professor Kelland's scientific papers is his Memoir on the limits of our knowledge respecting the Theory of Parallels. He there deals with the subject now better known as absolute or non-Euclidean geometry. It would scarcely be possible to convey to those who have not busied themselves with pangeometry (or the geometry of pure reason as one might venture to call it, as opposed to the geometry of experience which is Euclid's) a full idea of the importance of this work of Kelland's, and of the evidence that it affords of his grasp of purely mathematical speculation. Suffice it to say that he reasons out correctly, and perhaps even more elegantly than is done in one of the last works on the subject,* the consequences of denying Euclid's "parallel axiom," or what is its equivalent, viz., the proposition that the sum of the three angles of any triangle is two right angles. It can be shewn by means of the properties of congruent figures, which, with all the consequences as

* Frischauf, "Elemente der Absoluten Geometrie."

to the nature of space that follow therefrom, are hereby assumed that—(1) The sum of the angles of any triangle can never exceed two right angles ; (2) If the sum of the angles of any one triangle is two right angles, then the sum of the angles in every triangle is two right angles. But independently of the theory of parallels, this is in substance as far as we can go. If we assume that the sum of the angles of any triangle is less than two right angles, then we arrive at the conclusion that this sum depends on the area of the triangle, the defect from two right angles being less the less the area, and the same for all triangles of the same area, consequently therefore proportional to the area of the triangle. The effect of this assumption on the theory of parallels is very remarkable. Defining parallels as straight lines in the same plane that do not intersect (this is not the definition adopted in recent books, such as that of Frischauf, above named, but that is a mere question of words), we find that there are an infinite number of straight lines passing through the same point all parallel to a given straight line ; that through one point on one of a pair of parallels only one straight can be drawn that makes the alternate angles equal ; that parallel straight lines are not equidistant ; that the locus of the points equidistant from a given straight line is not a straight but a curved line ; that equal parallelograms on the same base cannot be between the same parallels, and so on. All this, and much more, is shewn by Kelland to form part of a system of geometry as logical as Euclid's.

As far as can be gathered from the memoir, and the form of the demonstrations, all but the fundamental propositions (the mere idea in fact) is Kelland's own work. It is characteristic of the man that he was in the habit of treating this subject in his class lectures.

Clearness in dealing with the fundamental principles of mathematical science was one of the virtues of Kelland's thinking and teaching. His text-book on *Algebra* is distinguished over other text-books in present use by its attempts to give a rational account of the first principles of the subject. The same readiness to grasp a new elementary idea, and trace its consequences, is exemplified by the fact that he took up Quaternions with his class in the University, and so late as 1873 published, in conjunction with Professor Tait, an excellent elementary treatise on this branch of mathematics. (See the *Preface* to that work.)

The minds of most men stiffen with age, and after a certain period the faculty of reception in most disappears. It was evidently not so with Professor Kelland.

Alexander James Adie, Esq. By David Stevenson, M.I.C.E.

Alexander James Adie, Civil Engineer, son of the late Alexander Adie, F.R.S.E., the eminent optician, was born in Edinburgh in 1808. A course of study at the High School, and afterwards at the University of Edinburgh, prepared him for entering on an apprenticeship under Mr James Jardine, Civil Engineer, with whom he was afterwards associated in carrying out various works.

In 1836 he became Resident Engineer of the Bolton, Chorley, and Preston Railway, and communicated some interesting papers to the Institution of Civil Engineers regarding that work, particularly one on Skew Bridges.

On leaving Lancashire he removed to Glasgow to take charge of some of the colliery railways there, and ultimately became engineer and manager of the Edinburgh and Glasgow Railway, which post he resigned about 1863.

Mr Adie made a series of important experiments on the expansion of stone by heat, which he communicated to the Society in his paper entitled "The Expansion of Different Kinds of Stone from an Increase of Temperature, with a Description of the Pyrometer used in making the Experiments," which is published in vol. xiii. of the Transactions.

Mr Adie was elected a Member of the Society in 1846. He latterly retired to reside at Rockville, near Linlithgow, where he had an opportunity of cultivating his taste for horticulture and the fine arts, and of receiving visits from many who esteemed his friendship, and valued his accomplishments.

John Blackwood, Esq. By Principal Sir Alex. Grant, Bart.

John Blackwood, who died on the 29th October last, was for a long period one of the most widely known and highly esteemed worthies of Scotland. As head of the last remaining of the great

Edinburgh publishing houses, he held an eminent and conspicuous position; his name was known and honoured all over the world; the circle of his acquaintance included almost all the most distinguished writers of the day, many of whom were his close and intimate friends; and the chorus of regret uttered by the London newspapers on the occasion of his death showed how widely and how much he had been respected. His life was externally uneventful. He was born in 1818; was educated at the High School and University of Edinburgh; travelled for three years under an accomplished classical tutor upon the Continent; commenced learning the publishing business with Messrs Whitaker in London in 1839; took charge of a branch of Messrs Blackwood's in Pall Mall in 1840; returned to Edinburgh in 1845, and became editor of "Blackwood's Magazine;" and in 1850 became head of the publishing firm in George Street. Happily married, and pursuing with conscientious diligence and great success the interesting duties of his position, keenly enjoying both work and relaxation, entering with equal zest into manly exercises and the intellectual pleasure of literary and witty conversation, to which he himself was no mean contributor, ever unselfish and taking an interest in others, diffusing much happiness among those who came within his range, he continued to exhibit till three years ago, when his health began to fail him, a career of high usefulness and a lot that was singularly blest. He continued in harness to the last, and within a few hours of his death was still reading the manuscripts of authors. He became a Fellow of the Royal Society of Edinburgh in 1857. He was never a contributor to the "Proceedings" of the Society. This, however, was only in accordance with the rule which he had laid down for himself, which was to abstain from authorship, in order to be able to estimate dispassionately and free from all feeling of rivalry the productions of others. Socrates used to say of himself that in matters of philosophy he performed the obstetric function for the youth of Athens, helping into existence such conceptions as were worthy to live and come before the world. The same sort of function John Blackwood performed for literature in this country. He was singularly fitted both by nature and education for the duties of his office. His knowledge of *belles lettres*, as well as of mankind, was extensive, and he had a remarkable sagacity in discerning

and foreseeing in the works of new writers, not only what was likely to be acceptable to the public, but what was essentially good in itself. During his thirty-four years of editorship and his twenty-nine years of publishing, he is said to have hardly ever made a mistake, while he frequently accepted works which had been rejected by other publishers, because he saw their merit, and the event proved him to have been right. In business transactions he was at once prudent and liberal, and always exhibited the qualities of a perfect gentleman. The result was a goodly and brilliant galaxy of great names in literature, who were his clients, and whose immortal works were first brought before the world under his auspices. The Royal Society of Edinburgh, one of whose objects is the encouragement of literature, must ever honour one who has been so faithful and valuable a servant and minister of the muses. And this Society, together with Edinburgh and the country at large, must deplore the loss of John Blackwood, than whom few men could have been less well spared.

James Clerk-Maxwell. By Professor Tait.

[James Clerk-Maxwell, born in 1831, was the only son of John Clerk-Maxwell of Middlebie. His grandfather, Captain James Clerk, was a cadet of the old Scottish family of Clerk of Penicuick, being a younger brother of Sir John Clerk of Penicuick. Captain James Clerk had two sons and a daughter—the Right Hon. Sir George Clerk of Penicuick, Bart., the above John Clerk-Maxwell, and Isabella, who married James Wedderburn, Solicitor-General of Scotland. Sir George Clerk succeeded to the estate of Penicuick, and the younger brother, John, to the estate of Nether Corsock, part of the estate of Middlebie. This estate had come into the family through the marriage in a former generation of a cousin of the Penicuicks with a Miss Maxwell. Their daughter married Sir George Clerk (grandfather of the present baronet) and was Lady Clerk-Maxwell. John Clerk assumed the name of Maxwell on succeeding to the property, which by the entail of Penicuick could not be held by the owner of that estate. John Clerk-Maxwell was called to the Scottish bar, but seldom practised, and he was a well-known member of this Society. He lost his wife soon after his

marriage, and lived a retired life, devoting himself to the care of his estates and the education of his son.]

When I first made Clerk-Maxwell's acquaintance about thirty-five years ago, at the Edinburgh Academy, he was a year before me, being in the fifth class while I was in the fourth.

At school he was at first regarded as shy and rather dull; he made no friendships, and he spent his occasional holidays in reading old ballads, drawing curious diagrams, and making rude mechanical models. His absorption in such pursuits, totally unintelligible to his schoolfellows (who were then quite innocent of mathematics), of course procured him a not very complimentary nickname, which I know is still remembered by many Fellows of this Society. About the middle of his school career, however, he surprised his companions by suddenly becoming one of the most brilliant among them, gaining high, and sometimes the highest, prizes for Scholarship, Mathematics, and English verse composition. From this time forward I became very intimate with him, and we discussed together, with school-boy enthusiasm, numerous curious problems, among which I remember particularly the various plane sections of a ring or *tore*, and the form of a cylindrical mirror which should show one his own image *unperverted*. I still possess some of the MSS. which we exchanged in 1846 and early in 1847. Those by Maxwell are on "The Conical Pendulum," "Descartes' Ovals," "Meloid and Apioid," and "Trifocal Curves." All are drawn up in strict geometrical form and divided into consecutive propositions. The three latter are connected with his first published paper, communicated by Forbes to this Society and printed in our "Proceedings," vol. ii., under the title "On the Description of Oval Curves, and those having a plurality of foci" (1846).

At the time when these papers were written he had received no instruction in Mathematics beyond a few books of Euclid, and the merest elements of Algebra.

The winter of 1847 found us together in the classes of Forbes and Kelland, where he highly distinguished himself. With the former he was a particular favourite, being admitted to the free use of the class apparatus for original experiments. He lingered here behind most of his former associates, having spent three years at the University of Edinburgh, working (without any assistance or supervision)

with physical and chemical apparatus, and devouring all sorts of scientific works in the library.* During this period he wrote two valuable papers, which are published in our "Transactions," on "The Theory of Rolling Curves," and "On the Equilibrium of Elastic Solids." Thus he brought to Cambridge in the autumn of 1850 a mass of knowledge which was really immense for so young a man, but in a state of disorder appalling to his methodical private tutor. Though that tutor was William Hopkins, the pupil to a great extent took his own way; and it may safely be said that no high wrangler of recent years ever entered the Senate-House more imperfectly trained to produce "paying" work than did Clerk-Maxwell. But by sheer strength of intellect, though with the very minimum of knowledge how to use it to advantage under the conditions of the examination, he obtained the position of Second Wrangler, and was bracketed equal with the Senior Wrangler in the higher ordeal of the Smith's Prizes. His name appears in the Cambridge "Calendar" as Maxwell of Trinity, but he was originally entered at Peter-House, and kept his first term there, in that small but most ancient foundation which has of late furnished Scotland with the majority of the Professors of Mathematics and Natural Philosophy in her four universities.

In 1856 he became Professor of Natural Philosophy in Marischal College, Aberdeen; in 1860, Professor of Physics and Astronomy in King's College, London. He was successively Scholar and Fellow of Trinity; and was elected an Honorary Fellow of Trinity when he finally became, in 1871, Professor of Experimental Physics in the University of Cambridge. There can be no doubt that the post to which he was ultimately called was one for which he was in every way pre-eminently qualified; and the Cavendish Laboratory, erected and furnished under his supervision, remains as remarkable a monument to his wide-ranging practical knowledge and theoretical skill as it is to the well-directed munificence of its noble founder.

If the title of mathematician be restricted (as it too commonly is)

* From the University Library lists for this period it appears that Maxwell perused at home Fourier's *Théorie de la Chaleur*, Monge's *Géométrie Descriptive*, Newton's *Optics*, Willis's *Principles of Mechanism*, Cauchy's *Calcul Différentiel*, Taylor's *Scientific Memoirs*, and many other works of a high order. Unfortunately no record is kept of books consulted in the reading-room.

to those who possess peculiarly ready mastery over symbols, whether they try to understand the significance of each step or no, Maxwell was not, and certainly never attempted to be, in the foremost rank of mathematicians. He was slow in "writing out," and avoided as far as he could the intricacies of analysis. He preferred always to have before him a geometrical or physical representation of the problem in which he was engaged, and to take all his steps with the aid of this: afterwards, when necessary, translating them into symbols. In the comparative paucity of symbols in many of his great papers, and in the way in which, when wanted, they seem to grow full-blown from pages of ordinary text, his writings resemble much those of Sir William Thomson, which in early life he had with great wisdom chosen as a model.

There can be no doubt that in this habit, of constructing a mental representation of every problem, lay one of the chief secrets of his wonderful success as an investigator. To this were added an extraordinary power of penetration, and an altogether unusual amount of patient determination. The clearness of his mental vision was quite on a par with that of Faraday; and in this (the true) sense of the word he was a mathematician of the highest order.

But the rapidity of his thinking, which he could not control, was such as to destroy, except for the very highest class of students, the value of his lectures. His books and his written addresses (always gone over twice in MS.) are models of clear and precise exposition; but his *extempore* lectures exhibited, in a manner most aggravating to the listener, the extraordinary fertility of his imagination.

During his undergraduateship in Cambridge he developed the germs of his future great work on "Electricity and Magnetism" (1873) in the form of a paper "On Faraday's Lines of Force," which was ultimately printed in 1856 in the "Trans. of the Cam. Phil. Soc." He showed me the MS. of the greater part of it in 1853. It is a paper of great interest in itself, but extremely important as indicating the first steps to such a splendid result. His idea of a fluid, incompressible and without mass, but subject to a species of friction in space, was confessedly adopted from the analogy pointed out by Thomson in 1843 between the steady flow of heat and the phenomena of statical electricity.

In recent years he came to the conclusion that all such analogies,

depending as they do on Laplace's equation, were best symbolised by the quaternion notation with Hamilton's ∇ operator; and in consequence, in his work on electricity, he gives the expressions for all the more important physical quantities in their quaternion form, though without employing the calculus itself in their establishment. I have discussed in another place ("Nature," vol. vii. p. 478) the various important discoveries in this remarkable work, which of itself is sufficient to secure for its author a foremost place among natural philosophers. I may here state that the main object of the work is to do away with "action at a distance," so far at least as electrical and magnetic forces are concerned, and to explain these by means of stresses and motions of the medium which is required to account for the phenomena of light. Maxwell has shown that, on this hypothesis, the velocity of light is the ratio of the electro-magnetic and electro-static units. Since this ratio, and the actual velocity of light, can be determined by absolutely independent experiments, the theory can be put at once to an exceedingly severe preliminary test. Neither quantity is yet fairly known within about 2 or 3 per cent., and the most probable values of both certainly agree more closely than do the separate determinations of either. There can now be little doubt that Maxwell's theory of electrical phenomena rests upon foundations as secure as those of the undulatory theory of light. But the life-long work of its creator has left it still in its infancy, and it will probably require for its proper development the services of whole generations of mathematicians.

The next in point of date of Maxwell's greatest works is his "Essay on the Stability of Saturn's Rings," which obtained the Adams' Prize in 1859. In this admirable investigation he shows that it is dynamically impossible that these rings can be solid, and also that they cannot be continuous liquid masses; the only other available hypothesis, viz., that they consist of multitudes of discrete parts, each a satellite, must therefore be the correct one.

Another subject which he treated with great success, as well from the experimental as from the theoretical point of view, was the Perception of Colour, the Primary Colour sensations, and the Nature of Colour Blindness. His earliest paper on these subjects bears date 1855, and the seventh has the date 1872. He received the Rumford Medal from the Royal Society in 1860,

"For his Researches on the Composition of Colours, and other optical papers." Though a triplicity about colour had long been known or suspected, which Young had (most probably correctly) attributed to the existence of three sensations, and Brewster had erroneously* supposed to be objective, Maxwell was the first to make colour-sensation the subject of actual measurement. He proved experimentally that any colour C (given in intensity of illumination as well as in character) may be expressed in terms of three arbitrarily chosen standard colours, X, Y, Z, by the formula

$$C = aX + bY + cZ.$$

Here a, b, c are numerical coefficients, which may be positive or negative; the sign = means "matches," + means "superposed," and - directs the term to be taken to the other side of the equation.

The last of his greatest investigations bore on the Kinetic Theory of Gases. Originating with D. Bernoulli, this theory was advanced by the successive labours of Herapath, Joule, and particularly of Clausius, to such an extent as to put its general accuracy beyond a doubt. But by far the greatest developments it has received are due to Maxwell, part of whose mathematical work has recently been still further extended in some directions by Boltzmann. In this field Maxwell appears as an experimenter (on the laws of gaseous friction) as well as a mathematician. His two latest papers deal with this branch of physics; one is an extension and simplification of some of Boltzmann's chief results, the other treats of the kinetic theory as applied to the motion of the radiometer.

He has written an admirable text-book of the "Theory of Heat," which has already gone through several editions, and a very excellent elementary treatise on "Matter and Motion." (See, again, "Nature," vol. xvi. p. 119.) Even this, like his other and larger works, is full of valuable matter, worthy of the most attentive perusal not of students alone but of the very foremost scientific men.

* All we can positively say to be erroneous is some of the principal arguments by which Brewster's view was maintained, for the subjective character of the triplicity has not been absolutely *demonstrated.*

Of his other scientific work, which extended over the whole range of physics, I may specially mention the following papers :—

On the transformation of surfaces by bending, "Camb. Phil. Trans.," 1854.

The discovery of the production of double refraction in viscous liquids ("Proc. R.S.," 1873), a late consequence of some of the results of his early paper of 1850.

A general theory of optical instruments, "Quart. Journ. of Math.," 1858.

On reciprocal figures, frames, and diagrams of forces, "Trans. R.S.E.," 1872. For this paper he obtained the Keith Prize.

His share in the construction of the British Association units of electric resistance, and in the admirable reports of the committee. Also his experimental verification of Ohm's law.

For further particulars recourse must be had to the Royal Society's Catalogue of Scientific Papers.

To these may now be added his numerous contributions to the latest edition of the "Encyclopædia Britannica"—Atom, Attraction, Capillarity, &c.; and the laborious task of preparing for the press, with copious and very valuable original notes, the "Electrical Researches of the Hon. Henry Cavendish." This work has appeared only within a month or two, and contains many singular and most unexpected revelations as to the early progress of the science of electricity.

The works which we have mentioned would of themselves indicate extraordinary activity on the part of their author, but they form only a fragment of what he has published; and when we add to this the further statement, that Maxwell was always ready to assist those who sought advice or instruction from him, and that he has read over the proof-sheets of many works by his more intimate friends (enriching them by notes, always valuable and often of the quaintest character), we may well wonder how he found time to do so much.

Maxwell's early skill in versification developed itself in later years into real poetic talent. But it always had an object, and often veiled the keenest satire under an air of charming innocence and *naive* admiration. No living man has shown a greater power of conden-

sing the whole substance of a question into a few clear and compact sentences than Maxwell exhibits in his verses. As an exceedingly good example of his style we may quote the lines written for the portrait of Cayley, now in Trinity College, Cambridge.

"O wretched race of men, to space confined!
What honour shall ye pay to him whose mind
To that which lies beyond hath penetrated?
The symbols he hath formed shall sound his praise,
And lead him on through unimagined ways
To conquests new in worlds not yet created.

"First, ye determinants in ordered row
And massive column ranged before him go,
To form a phalanx for his safe protection.
Ye powers of the nth roots of -1,
Around his head in endless cycles run,
As disembodied spirits of direction.

"And you ye undevelopable scroles,
Above the host wave your emblazoned rolls,
Ruled for the record of his bright inventions.
Ye cubic surfaces, by threes and nines,
Draw round his camp your seven-and-twenty lines,
The seal of Solomon in three dimensions.

"March on, symbolic host, with step sublime,
Up to the flaming bounds of space and time;
There halt, until, by Dickenson depicted
In two dimensions, we the form may trace
Of him whose mind, too large for vulgar space,
In n dimensions flourished unrestricted."

Other exquisite specimens are given in "Nature:" especially good is his "Lecture to a Lady on Thomson's Reflecting Galvanometer." One of the few others which have been printed was secured by John Blackwood for his Magazine, where it appeared under the title "British Association, 1874," in November of that year.

It is to be hoped that these scattered gems may be collected and published, for they are of the very highest interest, as the work during leisure hours of one of the most piercing intellects of modern times. Every one of them contains evidence of close and accurate thought, and many are in the happiest form of epigram.

I cannot adequately express in words the extent of the loss which his early death has inflicted not merely on his personal friends, on this Society, on the University of Cambridge, on the whole scientific world, but also, and most especially, on the cause of common sense,

of true science, and of religion itself, in these days of much vain-babbling, pseudo-science, and materialism. But men of his stamp never live in vain; and in one sense at least they cannot die. The spirit of Clerk-Maxwell still lives with us in his imperishable writings, and will speak to the next generation by the lips of those who have caught inspiration from his teachings and example.

Scotland may well be proud of the galaxy of grand scientific men whom she numbers among her own recently lost ones; yet even in a company which includes Brewster, Forbes, Graham, Rowan Hamilton, Rankine, and Archibald Smith, she will assign a place in the very front rank to James Clerk-Maxwell.

Dr Thomas Richardson Colledge.

Dr Thomas Richardson Colledge died on the 28th of October at Lauriston House, Cheltenham, in the eighty-third year of his age. He was a pupil of Sir Astley Cooper, and entered upon his profession sixty-two years ago; nor did he wholly relinquish his practice until 1878. To him, during his practice in Canton and Macao, belongs the merit of originating the first infirmary for the indigent Chinese, which was called after him "Colledge's Ophthalmic Hospital." He was also the founder of the Medical Missionary Society in China, and continued to be president of that society to the time of his death—a period of forty-two years. He laboured in Canton and Macao for more than twenty years, first under the Hon. East India Company, and then under the Crown as surgeon to His Majesty's Superintendents. On the abolition of the office he had held, and his consequent return to England, deep regret was expressed by the whole community, European and native, and a memorial of his services was addressed to Her Majesty the Queen in 1838 by the Portuguese of the neighbouring settlement of Macao. Lord Palmerston, in recognition of his services and merit, thought it right to award him an annuity. Dr Colledge took the degree of M.D. in 1839, and became F.R.C.P. of Edinburgh in 1840, and F.R.S. of Edinburgh in 1844. The last thirty-eight years of his life were spent in Cheltenham, where he won universal esteem by his courtesy and skill.

Elmslie W. Dallas, Esq. By General Robertson and Professor Piazzi Smith.

Elmslie William Dallas, the second son of William Dallas an underwriter at Llyod's, was born in London on 27th June 1809. He was educated at the Academy of Inverness, where he lived with his aunt, Mrs Sweetland, widow of General Sweetland; afterwards for a short time he attended classes at a commercial academy in London.

In his twenty-second year he decided to follow art as a profession, and was admitted a student of the Royal Academy in 1831. He completed his Academy studies in 1834.

The next six years (1834–1840) were spent on the Continent. During this time he resided a winter in Munich, nine months in Venice, and three years in Rome and its neighbourhood; he also spent several weeks at Dresden and Florence, and visited many other German, Flemish, French, and Italian cities. Several portfolios filled with highly-finished water-colour copies of the most celebrated pictures in the galleries he visited, and also with original drawings, sketches, and etchings, remain to testify the industry and skill with which during these six years the young artist pursued his studies.

In 1838 (æt. 29) he exhibited his first picture at the Royal Academy—it represented the interior of a Roman convent.

Soon after his return to England in 1840, he was employed by Herr Grüner to assist in the decoration of the garden pavilion at Buckingham Palace.

In 1841–42 he sent some pictures to the Royal Scottish Academy, which were well received and sold. In consequence of this success he resolved to settle in Edinburgh, and from 1842 until his death (*i.e.*, from his thirty-third to his seventieth year) he continued to reside there. For the next sixteen years (1842–58) he was a regular contributor to the annual exhibitions of the Royal Scottish Academy. His chief pictures were highly-studied interiors and mediæval subjects. There were also several landscapes, notably some views of the Campagna and its ruins. His last picture was exhibited in 1858 (æt. 50).

On 17th June 1846 he was appointed-assistant master of the architectural and ornamental class of the School of Design under

the Board of Manufactures. This appointment he held until 30th September 1858, when he was placed in retirement by the Treasury in order to carry into effect the affiliation of the school with the Science and Art Department of South Kensington. His connection with this school, therefore, extended over a period of twelve years (æt. 37–49).

On 3d March 1851 he was elected a Fellow of the Royal Society, to which he continued to belong until his death.

On 16th June 1859 (æt. 50) he married Jane Fordyce, eldest daughter of the late James Rose, W.S. Soon after his marriage he commenced the practice of photography as a profession, and applied the process of carbon-printing, with great success, to the illustration of books.

In 1870 (æt. 61) his health, which had previously been very good, was severely shaken by blood poisoning from bichromate of potash used in the process of carbon printing.

In 1872, when smallpox was prevalent in Edinburgh, he caught the infection from one of his assistants, and had a very severe attack of that disease. In the autumn of 1877, while on a visit to London, he had a very serious attack of typhoid fever, and never thoroughly recovered from the prostration of strength which followed the fever. The long-continued cold of the winter of 1878–79 tried him greatly. An attack of inflammation, brought on by a cold caught in January 1879, was the cause of his death, which took place at Dean-bank House on the 26th day of that month.

Such were the incidents in the uneventful but by no means unworthy life of Elmslie William Dallas. As regards pecuniary results it was a life of unsuccessful effort; but as regards the spirit in which the work of his life was done, and the intrinsic value and perfection of that work, E. W. Dallas's efforts to do well and thoroughly things worthy to be done, accomplished much that was admirable, in a manner that was most instructive and exemplary to all who had opportunities of observing the wealth of earnest lucid thought and the patient skilfulness of hand with which he worked out his results.

On 2d February, the Sunday following his funeral, the Rev. John M'Murtrie, speaking from the pulpit of St Bernard's, in which church Mr Dallas had been for upwards of ten years an elder,

said :—" His beautiful features, his grave almost sad expression, as of one who had fought life's battle and was wearied, were familiar to us all; but most of us could only guess at the worth, the truth, the goodness which lay under that reserved demeanour, for he shrank from prominent positions, and had that low estimate of his capacity for public affairs which often characterises the very best. But whatever he undertook he carried out thoroughly. His was a pure and chastened life; its brightest side not seen by the world, but shining in his own home for those who were dearest to him. In severe illnesses—of which he had several—he was gentle and patient; but I never knew how brave he was till I saw him face the last enemy without fear, in lowly trust in his blessed Saviour."

Professor Piazzi Smyth, who knew him well and had intercourse with him more or less frequent for a period of upwards of thirty years (from 1846 until his death), thus bears testimony to what he terms " the high calibre of his character."

E. W. Dallas was (the Professor says) undoubtedly a remarkable man: gifted with a *naive* simplicity of mind and thorough goodness of heart, as well as with no uncertain abilities of head and hand. Each singly of an admirable kind, and collectively very rarely found combined in the same individual. Yet so modest and retiring withal, was the possessor of them, that these rare abilities were little known.

The mere extent of his knowledge in the fine arts, and the great number of his acquisitions in the exact sciences, were, to say the least of them, very noticeable. But still more noticeable was the thorough soundness of his knowledge of every subject he had studied; so that I find now, on looking back through the years that are gone, this far higher commendation for him than any amount of local success or of temporary celebrity; viz., that almost whatever he said, or did, at any time, has stood: having been proved by subsequent experience to be true; and I have never regretted any moments I have spent in his company, either listening to his opinions or discussing his views.

I first met him in his capacity as a teacher of the architectural and ornamental class in the Trustees' School of Design, in the Royal Institution. The outlines to be copied were of large size, of classical severity, and yet not without poetical feeling; and he taught with

success, both morning and evening, a class of between seventy and eighty youths.

Equally skilful was he at home in modelling exquisite ideal forms in clay or wax; or in carving in wood, some of Nature's choicest leaves and flowers, with a delicacy of imitation which made a charming approach to the beauty of the originals.

At other times he would take up his palette and either paint landscapes from notes of former continental travel; or produce figures, usually of the *genre* kind, which testified to his possession of considerable powers of imagination, and of a lively memory well stored with reminiscences of extensive mediæval reading. Here, then, we have at once powers of multifarious work, extending over a very considerable range of the fine arts; enough of itself to have fitted out most successfully for the battle of life, many an aspirant for fame. But to all these artistic faculties, Elmslie W. Dallas added mastery of not a few branches of hard science; as thus—

1. He wrote a book on applied Geometry for the use of the School of Design, showing complete knowledge of the latest continental developments of the subject.*

2. He prepared papers on the optical mathematics of lenses.

3. He entered at one time with zeal and fervour into the casting, grinding, and polishing of the specula of reflecting telescopes.

4. He made experiments in improving and adapting compound microscopes to special subjects of minute anatomy.

5. He possessed a considerable range of chemical knowledge, and made many experiments, both on large and small scale, in crystallogenesis.

* In a report upon this treatise submitted to the Board of Manufactures on 17th June 1860, the late Professor Kelland writes :—" Regarded as a book of reference, which shall contain all the more important solutions of the ordinary problems of Practical Geometry, this treatise deserves very high commendation. The constructions adopted by the author seem in all cases to have been well selected; and the arrangement, founded on a classification of results, is eminently adapted to afford facility of reference."

The Professor, however, reported that, considered as an educational treatise, he did not think its arrangement suitable for the instruction of youth; and the result has confirmed this judgment. As a class-book, Mr Dallas's treatise has been superseded in the School of Art by a much less elaborate and more elementary little book compiled by Mr J. S. Rawle, Headmaster of the Nottingham Government School of Art.

6. He prepared grandly illustrated papers on the minuter forms of microscopic infusoria.

7. Long before he adopted photography as a profession, and when very few persons in this country knew anything about it, he had become conversant with the then newly-born art in all its chemical, as well as its optical and mechanical details; and he had prepared, with his own hands, special and instantaneous apparatus for applying it, on the one hand to record sun-spots as shown by a telescope; and on the other hand, to picture microscopic images of his favourite forms of naviculæ.

Now, how could any ordinary man occupy himself with all these arts and sciences, without being more or less shallow in some, and proving an undesirable leader or adviser in others of them?

It would be impossible! and yet so conscientious a student and thorough a worker was E. W. Dallas, that he possessed skill and solid acquirements in them all. Without pretension or direct effort on his part, he was looked up to, as rather a notable authority, in all of them, by many persons who prosecuted only one or other single subject out of the many with which our late Fellow was conversant.

At a meeting of the first Edinburgh Photographic Society, established by the late Sir David Brewster, when a novel kind of landscape lens, invented by that very original genius the late Mr Sutton, was laid on the table, how the members in general were non-plussed! It was a fluid-corrected, achromatic, globular lens with "butterfly diaphragm" stop, and producing equal illumination and good definition over three times as wide an azimuthal angle as had ever before been obtained. Presently Elmslie W. Dallas entered the room and sat down in a quiet corner, when it was perfectly delightful to me (a non-professional looking on) to see how several of the best men in the room brought the lens to *him*, told him all their hopes, fears, and difficulties about it, and then hung expectant on his words as though they would prove infallible—and if he spoke at all, his words, on such a matter, might be accepted as infallible. For although, not only when questioned privately but also in public, he was often sufficiently discourseful, yet he could be silent when he chose; and would not let popular applause, or personal requests, or hope of gain move him to give out a single opinion on any

subject, further than he himself had examined into it, after his own thoroughgoing manner, and to the satisfaction of the special ideal aspirations of his own soul. And herein was the most individual trait of the man—the rare cast of mind which made him a most worthy member of the Royal Society of Edinburgh ; yet caused his worldly success in life, to fall far below his intrinsic worth and high capacities.

Gifted by nature with a sensitive soul, responsive to the love of abstract truth and appreciative of ideal beauty; ever inclined to be generous beyond his means, and quite incapable, amidst higher surroundings, of bestowing serious and concentrated attention on petty affairs, he worked at his profession (photography) in a manner regardless of cost ; and not so much for profit, as for the sake of the scientific interest he involuntarily felt in overcoming difficulties in the practice of the art. That he did, from such motives, procure the most marvellous lenses and the most elaborate apparatus ; that he tried, with patient and often long-protracted and expensive experiments, every new method in photography, was to his honour as a lover of science ; but was not to his advantage as a man of very limited means, whose income mainly depended on daily studio work of a more certain kind. And, more untowardly still for his success in securing an adequate income, this taste for perfection and power in all the objective of his art, was accompanied by a curious inner subjective state of mind,—by a kind of inward psychical craving, perpetually urging him to desire, that his knowledge of whatever he touched, should be if possible more than perfect : persuading him too, that in order to know thoroughly any particular thing in nature, he should not only know and handle the thing itself, but that to be quite certain about it, he ought also to become similarly acquainted with everything else existent which, though outwardly excessively like, was not in reality the very thing itself; and might in consequence, at some time or other, possibly deceive the unwary. Under the pure, but exacting, domination of which idea, carried as it was by him to an inordinately high degree, he appeared at last to think that, in the conduct of his scientific inquiries, his chief duty consisted rather in finding and proving a negative ; than in either establishing any positive result, or in securing opportunities for the most brilliant mercantile success. Had he been heir to a large fortune he

would have rendered services of the most invaluable kind to the science of the age he lived in; for with his eminent skill, perseverance, and capacity for untiring labour, joined *then* to ample pecuniary resources, he would have followed up most exhaustively all the *least* inviting paths of thought and experiment. And whenever he had traced the objects of his investigation, step by step, both back to their sources, and onward to their final outcome and practical application, according to his own high ideas of efficiency in research,—he would have been equally ready, if the result of his labours proved to be something good, true, and workable, to present it as a free gift to others; but if the contrary, to keep all the disappointment to himself. And no self-sacrifice in thought or work would ever have weighed with him for a moment, if by such devotion he foresaw that the road to future success, through any very difficult labyrinth, would be made safer and straighter for others. But without any adventitious aids of either fortune or favour, E. W. Dallas did, in fact, to a very great extent, fulfil the noble part for which he was in a manner designed, and specially endowed, by nature. And living as he did, conscientiously, day by day such a life, his soul could not but be advancing *pari passu*, and maturing itself to the end of his appointed time here below.

His own work is finished; but his rare example has, without doubt, even unknown to himself, kindled the spark of progress and self-improvement in many another mind that was around him; and his noble qualities, not less *excelsior* in aim, but more practically applied, may reappear in his own family, in another generation, as well as in a different field of labour.*

Dr J. G. Fleming. By Dr Andrew Wood, Edinburgh.

Dr John Gibson Fleming, who for many years occupied a prominent position in Glasgow as a medical practitioner, at first in general practice and latterly as a consultant, was born there on the

* E. W. Dallas leaves behind him a widow, a son, and two young daughters (twelve and five years of age). In the term ending July 1879, his only son James passed out of the Royal Academy of Woolwich, first of the commission class of Cadets. Besides the Pollock gold medal and a sword of honour for general good conduct, he received prizes for excellence in five special subjects. James Dallas is now a Lieutenant in the Royal Engineers.

2d December 1809. He was sprung from a family which had been long settled in Glasgow, and whose names are often mentioned in its annals. He received the whole of his early education at the High School of Glasgow, and afterwards at the University. Subsequently he prosecuted his medical studies in the University under such eminent teachers as Thomas Thomson and Graham in chemistry, Jeffray in anatomy, Burns in surgery, Bodham in physic, Hooker, the elder, in botany, &c. After taking his degree of M.D. in 1830 he spent some time in Paris and other continental cities. Returning home, he in 1833 became a Fellow of that ancient body, the Faculty of Physicians and Surgeons of Glasgow. Ere long he succeeded in establishing himself in an extensive practice, and ever since then down to his decease he continued to practise in Glasgow with great acceptance. It may show the estimation in which he was held by his professional brethren of the Faculty that he was exceptionally elected again and again as its President. This estimation was still further shown in 1862 when he was elected its representative in the General Council of Medical Education and Registration. This honourable and responsible office he continued to hold for fifteen years, when he resigned, much to the regret as well of his colleagues in the council as of the fellows of the faculty. In that council he was not a very frequent speaker—for he did not lay claim to oratorical gifts—but when he did speak what he said was terse and marked by shrewd common sense and judiciousness, so that he was always listened to attentively by the council, amongst whom he was greatly esteemed.

He made few contributions to medical literature. In 1862, however, he published "Medical Statistics of Life Assurance, being an Inquiry into the causes of Death among Members of the Scottish Amicable Assurance Society from 1826 till 1860." In this work, which was very carefully prepared, he gave an analysis of the diseases which had proved fatal to the assured as compared with the general mortality. This was a valuable contribution to the medical department of life assurance, and was well calculated to aid the medical referees of assurance companies in the selection of lives for assurance.

Dr Fleming had ample opportunities of giving vent to his philanthropic feeling in the management of various charitable institutions

in his native city, especially the Royal Infirmary, in which, by the way, he had served for many years as surgeon and physician, and in which he introduced many improvements.

Down to the period of his last fatal attack of typhoid fever, by which he was cut off on the 2d of October 1879 after a brief illness, Dr Fleming continued to perform with wonted energy and ability his duties, professional and otherwise, and may truly be said to have died in harness. His loss was greatly regretted by a large circle of patients, by whom he was regarded with esteem and affection, and by the public of Glasgow generally. In conclusion, it may be truly said that the history of Dr Fleming is that of a career modest and uneventful, but useful, honourable, and successful to the last.

Arthur Hay, Marquis of Tweeddale.

By Robert Gray, Esq.

Arthur Hay, 9th Marquis of Tweeddale, F.R.S., and president of the Zoological Society of London, was born on the 9th November 1824. He was the second son of his father, the 8th Marquis, who was a distinguished soldier, and the first agriculturalist of his time. Having in his eighteenth year obtained a commission in the Grenadier Guards, Lord Arthur Hay, as he was then called, on attaining the rank of Captain about a year afterwards, went out to India as A.D.C. to his father, who was Commander-in-Chief at Madras. At the end of a few years service in this capacity he was appointed A.D.C. to the Governor-General Lord Hardinge, and served under him in the Sutlej campaign of 1845–46. He was present at the decisive battle of Sobraon, and on the conclusion of the Treaty, by which the British became possessors of the hill territory west of the Sutlej and Cashmere, he, with several of his brother officers, visited this part of the Himalayas—a journey which afforded him ample opportunities for prosecuting his favourite study, and making a large collection of the birds of the country.

During his residence in India, Lord Arthur Hay formed the acquaintance of the late Dr Jerdon, a distinguished Eastern naturalist, who was in the early part of his life Assistant-Surgeon at Fort St George. Subsequently he was on terms of intimacy with other eminent naturalists; but he does not appear to have published more

than two scientific papers previous to 1866. These two appeared in the *Madras Journal*, one in 1844–45 entitled "Descriptions of some supposed new or imperfectly described species of Birds," the other in 1847 entitled "Notice of the Habits of the Large Indian Boa or Rock Snake."

In 1862 Lord Arthur Hay assumed the title of Lord Walden on the death of his elder brother Lord Gifford, and for the next four years of his life was almost entirely occupied with his military and other duties, as indeed he had been for many years previously. He was present with his regiment at the various battles fought during the Crimean war, and passed through the whole of that memorable campaign with distinction. He took part in the siege of Sebastopol, and received, among other honours, the medal and clasp for the war, the Sardinian medal of valour, the Turkish war medal, and the fifth class of the order of the Medjidie. He was ultimately promoted in 1860 to a Colonelcy in the Grenadier Guards, but was placed on half pay in 1863. In 1866, after becoming a Lieutenant-Colonel of the 17th Lancers, he finally retired from the army, and betook himself to scientific pursuits. For the next ten years he resided at Chislehurst, during which period he contributed a most important series of ornithological papers to the "Proceedings and Transactions of the Zoological Society," the "Annals of Natural History," "Rowley's Ornithological Miscellany," "The Ibis," and other periodical magazines—many of these papers specially referring to the birds of India and the Eastern Archipelago.

Upon the death of Sir George Clerk in 1868, Lord Walden was elected President of the Zoological Society of London—an office in which he discharged his duties in the most efficient manner until his death.

Lord Walden succeeded to the peerage and estates on the death of his father in 1876; and at that time, having taken up his residence at the family seat, Yester, in Haddingtonshire, he entered upon the investigation of the avi-fauna of the Philippine Islands, at which subject he worked with extraordinary zeal, the result being a most valuable series of papers, thirteen in number, which appeared in the "Proceedings of the Zoological Society," the last of which was finished but a day or two before the author's death.

The papers of greatest value written by Lord Tweeddale appeared

between 1867 and 1878. These relate almost exclusively to descriptions, with figures, of new species of birds from various parts of the world, and are looked upon as the most important contributions to ornithological science that have been published during the same period in this or any other country. No one, indeed, can look upon the masterly work of Lord Tweeddale without feeling that by his sudden and premature death an irreparable loss has fallen upon the science to which he was devoted, and that many years must pass before ornithologists cease to deplore his untimely removal.

In 1877 Lord Tweeddale published fifteen separate papers on ornithological subjects, and in the following year about the same number—the fourteenth and last having, as already mentioned, been finished only a few days before his death. His loss, therefore, came upon the scientific world at a time when his writings were being regarded with a peculiar interest, and when he himself was everywhere being recognised as the most able ornithologist of his day.

Lord Tweeddale died at Walden Cottage, Chislehurst, on the 29th December 1878. His collections of birds, which are of great value, being the repository of a large number of type species described in the papers referred to, together with his valuable library of scientific works, are bequeathed to his nephew, Mr R. Wardlaw Ramsay, himself an ornithologist of considerable note.

Dr James M'Bain. By Robert Gray, Esq.

Dr James M'Bain was born at Logie, in Forfarshire, in November 1807. After having spent some years at the parish school of Kirriemuir, and about three years as an apprentice to a local surgeon, he entered upon the study of medicine at the University of Edinburgh in 1823. Three years later, namely, in March 1826, he passed his examination at Surgeons' Hall, and received his diploma when little more than nineteen years of age. About this time he removed to St Andrews, where he spent upwards of twelve months; and in the autumn of 1827 he was appointed assistant-surgeon to H.M.S. "Undaunted," just then commissioned to proceed to India with the newly-appointed governor, Lord William Bentinck. During this and a subsequent voyage in the same ship in 1829, to the Azores and Cape de Verde Islands, Dr M'Bain had but

limited opportunities of cultivating his taste for natural history pursuits, but such leisure as he enjoyed enabled him to collect various notes which, although not published at the time, became useful to him in after life.

In 1832, Dr M'Bain, in the capacity of assistant-surgeon, joined the "Investigator," a surveying ship, under the command of Captain Thomas, who was at that time employed in a survey of the Shetland Islands. This survey was completed in 1834, and was followed by a survey of the Orkney Islands, during which Dr M'Bain and Captain Thomas prosecuted a series of successful dredgings in deep water between the two groups of islands, as well as along their shores. Much interesting information and material resulted from their joint labours, extending over a period of sixteen years, and was freely communicated to Messrs Forbes and Hanley and Dr Harvey, who were then engaged in bringing out their important works on Molluscous Animals and British Seaweeds.

After settling for some years at Elie, in Fifeshire, and subsequently at Leith and Trinity, Dr M'Bain continued to devote his time and attention to the investigation of the marine fauna of the Firth of Forth; and while engaged in this he was the friend and frequent companion of Dr Fleming, Prof. Goodsir, Dr Strethill Wright, and other naturalists, who often accompanied him in his dredging excursions. During these years he took an active interest in the proceedings of the Royal Physical Society, of which society he was twice president, and contributed many papers of interest, which appeared at intervals from 1859 to the time of his death. He also contributed to a Topographical work by the Rev. W. Wood, Elie, entitled the "East Neuk of Fife," an important catalogue of the *Mollusca* of the Firth of Forth, embracing 344 species—244 of which were collected by himself; and while he was in the midst of such labours his friends had reason to regret that the state of his health and retiring modesty prevented him undertaking some independent work in which he might have done justice to his powers. He had an extensive knowledge of comparative anatomy, having at one period of his life enjoyed the advantage of studying under Professor Owen of London—a training to which much of the thoroughness of his knowledge as a naturalist may perhaps be attributed.

In private life Dr M'Bain was much esteemed by a large circle of friends. A man of extensive reading, amiable and unobtrusive in manner, he quietly prosecuted his practical work as a naturalist uninfluenced by any of the various theories which are not fully supported by facts. One scientific fact, indeed, to use his own words, was to him worth all the poetry in the world. He took a great interest in the scientific studies of young naturalists, and was at all times ready to give them the benefit of his counsel and wide experience. Many such students now mourn his loss in distant lands.

Dr M'Bain died, after a painful illness of some months' duration, at Trinity, near Edinburgh, on 21st March 1879.

Professor James Nicol. By Professor Archibald Geikie.

In the death of Professor James Nicol the Society has to regret the loss of one who served to link the present generation of geologists with the early leaders of the science in this country. Trained in this university under Jameson, he imbibed that love for the mineralogical side of geology which distinguished his career. His earliest scientific publication—an essay on the geology of his native county of Peebles—was awarded a prize by the Highland Society, and was issued in their "Transactions." At the time of its appearance very little had been added to the original observations of Sir James Hall, communicated to Hutton, and published in the "Theory of the Earth," regarding the structure and constitution of the so-called *schistus* or *killas*, forming the uplands of the south of Scotland. Mr Nicol, however, continued to devote himself to the investigation of this subject. He was the first to suggest that these rocks should be paralleled with some of the "Silurian" formations made known by the researches of Murchison; and in subsequent communications to the Geological Society of London he brought forward contributions to the unravelling of the complicated geology of these Silurian uplands of Scotland. At an early period of his life he published a small volume under the title of "Guide to the Geology of Scotland." Though chiefly compiled from the published memoirs of previous observers, it was a meritorious and useful work, giving within a small compass a trustworthy digest of

all that was known at the time upon the subject. A more important work was his well-known "Manual of Mineralogy" which has long been a standard book of reference.

His papers giving promise of much ability, he was appointed to the important office of Assistant Secretary of the Geological Society of London, where he came into intimate relations with the leading geologists of the day. Afterwards he became Professor of Geology at Queen's College, Cork—an office he soon vacated for the chair of natural history in the Aberdeen University, in the discharge of the duties of which he has spent the larger part of his scientific career.

For the last fifteen years he published scarcely any scientific papers, devoting his time principally to the business of the College, in which he took an active interest. During summer, however, he was in the habit of making excursions into the Highland mountains, where he renewed his acquaintance with minerals and rocks, which retained their interest for him to the last. Retiring in disposition, and latterly in somewhat enfeebled health, he allowed himself almost to drop out of the acquaintance of his fellow geologists, who rarely had an opportunity of seeing him save by visiting him at Aberdeen, or joining him in one of his Highland rambles. His unfailing kindliness and readiness to help others greatly endeared him to his students.

Dr John Smith. By Dr Batty Tuke.

Dr John Smith was born in the year 1798. His father combined the business of brassfounder and farmer, renting the Calton Hill and a few adjacent fields. It may be interesting to place on record that Dr Smith's father's mother was born in 1685, the last year of the reign of Charles the Second. He was educated at Heriot's Hospital, by the Governor of which institution he was recommended to Dr George Wood, son of the well-known Dr Alexander Wood, as an apprentice. He took the degree of Doctor of Medicine in the Edinburgh University in the year 1822, and became a Fellow of the Royal College of Physicians in 1833. After graduation he acted as Dr Wood's assistant, and eventually succeeded to his practice, which included the management of the Saughton Hall Asylum for the

Insane. He was also visiting physician to the old Charity Workhouse and City Bedlam in the Forrest Road. Dr Smith was elected President of the Royal College of Physicians in 1865. He died February 4, 1879. Dr Smith's contributions to the literature of medicine were not numerous, but were marked by extreme conscientiousness of observation. His most important papers are "An Account of Dysentery as it occurred in the Edinburgh Charity Workhouse during the years 1832 and 1833," and "Cases of Mental Derangement terminating fatally, with the Appearances disclosed by Inspection," both published in the Edinburgh Medical and Surgical Journal. Dr Smith was best known in his connection with the treatment of insanity, and he gained a considerable reputation in that special line of practice. It cannot be said that he displayed any great originality, his character being chiefly marked by accuracy, conscientiousness, and solidity, which qualities, however, added to great gentleness of disposition, procured him the respect and esteem of a large circle of friends, and the confidence of his professional brethren.

Sir Walter Calverley Trevelyan, Bart. By Dr Benjamin Ward Richardson, F.R.S.

Sir Walter Calverley Trevelyan, Bart., of Wallington in Northumberland, Nettlecombe in Somersetshire, Seaton in Devonshire, and Trevelyan in Cornwall, is another of the Fellows whom the Royal Society of Edinburgh has lost during the past year. The late Sir Walter was a scholar of the most refined taste and varied learning. His mind through all the stages of his long and active life was devoted to the acquirement and improvement of natural knowledge. He was born on the 31st of March 1797, his father being the fifth baronet of his line, and his mother a daughter of Sir Thomas Spencer Wilmot, Bart. Sir Walter commenced his university studies as an undergraduate at Oxford when he was about nineteen years of age, and in 1820 passed as Bachelor of Arts. Soon after this he visited the Faroe Islands, and wrote an account of them, including a record of their geology, vegetation, and climate. He also formed a collection of plants, making a fine herbarium,

which he presented in after years to the Botanical Museum at Kew.

In 1835 Mr Trevelyan married Paulina, the oldest daughter of the late Rev. Dr Jermyn, who lived until the year 1866.

After the British Association for the Advancement of Science had been founded in 1831, Mr Trevelyan took a deep interest in its progress. He served on the local committee of the Association when it met in Newcastle in the year 1838, and he was afterwards a member of the Council of that learned body. At the thirty-second meeting of the Association in the year 1862, he was elected one of the Vice-Presidents, his colleagues being Sir C. Lyell, Hugh Taylor, Isaac Lowthian Bell (Mayor of Newcastle), Nicholas Wood, the Rev. Temple Chevalier, and Mr (afterwards Sir William) Fairbairn.

Sir Walter came into possession of his estates and title in the year 1846, and from that time resided principally at his beautiful estate at Wallington, near to Cambo, Northumberland, a mansion of great historic note, and once the seat of a famous Jacobite, whose opinions cost him his life—Sir John Fenwick. He was elected Deputy-Lieutenant of the County in 1847, and in 1850 served the office of High Sheriff.

His time was much devoted to the improvement of agriculture and to the social amelioration of the condition of the people. He also took a deep interest in public affairs, and as far back as 1853 he became the first President of the United Kingdom Alliance for the suppression of the sale of intoxicating liquors, which office he continued to hold until his death.

In addition to his tastes for science, Sir Walter Trevelyan was a willing patron of the fine arts, and collected at Wallington some exquisite artistic works, in addition to a perfect museum of natural history. He was a Fellow of the Society of Antiquaries and a zealous antiquarian, and has left to the British Museum and the Society of Antiquaries valuable legacies. He was a clear and concise writer, and contributed several very useful papers on geological and botanical subjects. He was also a thoughtful and collected public speaker, who made every sentence he spoke tell, and who never wasted a sentence or, it may almost be said, a word.

In 1867 Sir Walter married, for the second time, Laura Capel, the daughter of Capel Lofft, Esq., of Troston Hall, Suffolk, who

survived him only for a few days. He had no issue, and his title has descended to his nephew, Sir Alfred W. Trevelyan of Nettlecombe, the present baronet. Wallington he bequeathed to his cousin, Sir Charles Trevelyan, K.C.B.

Sir Walter Trevelyan continued actively engaged in his various pursuits until March 1879. He suffered a very short illness, having been out a day or two before his death, and was occupied, indeed, with his correspondence on the morning of that day. He suffered, as it seemed, from a cold, accompanied with slight physical depression. In the course of March 23d he began suddenly to show signs of exhaustion, and sank into death without any continued sign of acute pain. He was in the eighty-third year of his age at the time of his death.

The late baronet was elected a Fellow of the Royal Society of Edinburgh in the year 1822.

Professor Heinrich Wilhelm Dove. By Alexander Buchan, M.A.

Professor Heinrich Wilhelm Dove was born at Leignitz, Silesia, on October 6th 1803, and at the age of eighteen passed from the schools of that town to the universities of Breslau and Berlin, where for the next three years he devoted himself assiduously to the study of mathematics and physics. In 1826 he took his degree of Doctor of Philosophy, his thesis on the occasion being an inquiry regarding barometric changes; and it is further significant of his future life-work that his first published memoir was a paper on certain meteorological inquiries relative to winds—these two subjects holding a first place in the great problem of weather-changes.

In the same year Dove entered on his public life as tutor, and in 1828 as Professor at Konigsberg, where he remained till 1829, being then invited to Berlin as Supplementary Professor of Physics. His strikingly clear-sighted, bold, and original intellect turned instinctively to that intricate group of questions in the domain of physics which comprise the science of meteorology, and his success in these fields as an original explorer was so marked and rapid that he soon achieved for himself a seat in the Royal Academy of

Sciences, and sometime thereafter was raised to the distinguished position of the chair of physics in the University of Berlin.

Among the scientific and fashionable circles of Berlin he took first rank as a lecturer, the combined qualities of accurate science, fine imagination, lucidity of style, commanding presence, and the extent over which his utterances were heard, marking him out as the Arago and Brewster of Germany. Germany showered on him in profusion those honours and offices which it gracefully and gratefully bestows on learning and science; and perhaps there is no learned or scientific society of note that has not Dove's name enrolled among its honorary members. After a protracted and hopeless illness he died on Friday April 4th 1879, in the seventy-sixth year of his age.

In the Royal Society's Catalogue of Scientific Papers the lists under Dove specify 234 memoirs, written between the years 1827–73. These show him to have been a successful worker and investigator in electricity, optics, crystallography, and in such practical matters as the metric systems of civilised nations. But it was to meteorological inquiries that he devoted his full strength and the whole powers of his mind, and by his herculean, but well-directed labours, he has written his name in large imperishable characters on the records of science.

His fame rests on the successful inquiries he carried out with a view to the discovery of the laws regulating atmospheric phenomena, which apparently were under no law whatever. The work he will be long best known by is his isothermals and isabnormals of temperature for the globe, in which work one cannot sufficiently admire the breadth of view which sustained and animated him as an explorer during the long toilsome years spent in its preparation. Equally characterised by breadth of view, and what really seemed a love for the drudgery of detail even to profuseness, when such drudgery appeared necessary or desirable in attaining his object, are various works on winds, the manner of their veering, and their relations to atmospheric pressure, temperature, humidity and rainfall, and the important bearings of the results on the climatologies of the globe; on storms and their connections with the general circulation of the atmosphere; the influence of the variations of temperature on the development of plants; and the cold weather of May—to which

may be added the valuable system of meteorological observations he gradually organised for Germany, and the many full discussions of these which he published from year to year.

It is no ordinary praise to pass on his work to say that those views he propounded, which subsequent researches are likely to modify materially, are those he arrived at by methods of investigations, necessarily defective, at the time. Thus, for instance, in inquiring into the law of storms, it was not in his power to work from isobaric charts, seeing that the errors of the barometers and their heights above the sea were only known in a very few cases. When we consider the condition in which he found man's knowledge of weather and the large accessions and developments it received from his hand, the breadth of his views on all matters connected with the science, and the well-directed patience, rising into high genius, with which his meteorological researches were pursued, there can be but one opinion, that these give Dove claims which no other meteorologist can compete with, to be styled "the father of meteorology."

Johann von Lamont. By Alexander Buchan, M.A.

Johann von Lamont was a Scotsman by birth, having been born in Deeside on the Balmoral estate in 1805, of one of the oldest of our Scottish families. At the age of seventeen he left Scotland, to which he never returned, in the prosecution of his studies in connection with the Roman Catholic Church. Whilst a faithful and zealous member of the clergy of that communion, it was to the Exact Sciences he devoted the full powers of his singularly energetic and penetrating intellect. His first contribution to science was published in 1829, in the twenty-fourth year of his age, the subject being the Motions of Encke's Comet, and from that date to 1870 the Royal Society's Catalogue of Scientific Papers enumerates no fewer than 107, ranging widely over the domain of physics, and several of which take their places as classics in the departments of science with which they deal.

His most extended work is his "Hand-book of Magnetism," published at Leipsic in 1867 as one of a series of works forming a general Encyclopedia of Physics, under the editorship of Karsten,

and in this department of knowledge he was one of the greatest authorities. In meteorology proper, the manner in which he presented and discussed the facts of observation of the diurnal barometric range, and the aqueous vapour of the atmosphere, and the theories he propounded therefrom, were eminently original, and will, we believe, always continue to be read, however much they may be modified or even overturned by future research. In astronomy, Professor Lamont's chief work was his Catalogues of Small Stars between 15° north and 15° south of the equator, being supplementary to the larger work under this head of Argelander and Bessel. As early as 1851 he demonstrated the existence of a decennial cycle in the diurnal range of the magnetic declination, which was more recently conclusively shown to correspond with the cyclical frequency and abundance of the sun-spots.

He was appointed Director of the Bogenhausen Observatory at Munich in 1835, and Professor of Astronomy in the University of Munich in 1852. He died at Bogenhausen early in the morning of Wednesday the 6th of August in the seventy-fourth year of his age.

Monday, 15th December 1879.

THE RIGHT HON. LORD MONCREIFF in the Chair.

The following communications were read:—

1. On the Expansion of Cast Iron while Solidifying. By J. B. Hannay, F.R.S.E., F.C.S., and Robert Anderson.

The fact that certain bodies expand on solidifying, as in the case of water, has long been well known, and this property has been recognised in some of the metals, owing to their filling the mould in which they are cast so as to reproduce the finest lines. The fact of their so doing is, however, only known qualitatively—no accurate measurements having, so far as we are aware, been recorded. The property being of great interest to ironfounders, we have undertaken a series of experiments to determine its real value—the materials being put at our disposal by Messrs M'Dowall, Steven, & Co., to whom we tender our best thanks. We used several methods, and

will show the reasons for selecting one as the most reliable. On pouring iron into a sand mould there is, at the moment of solidification some overflow; but no matter how tightly the sand was rammed, or to what temperature the mould was heated, the overflow varied so very much as to show that, as a method of measuring the expansion, pouring the iron into a sand mould was quite useless. The experiments conducted in this way showed an expansion of from 0·8 to 4·5 per cent., showing the method to be unreliable.

We then tried pouring the metal into a hollow sphere of iron whose capacity has been accurately determined. The sphere, however, seemed to yield in some parts, so that the overflow did not represent accurately the real expansion; but by weighing the filled sphere and the overflow, after we had arrived at an approximate value for the liquid iron's density, a more reliable estimate of the expansion was found. This method gave results varying from 4 to 5 per cent. of expansion, but the results were always low. The method which gave not only the most concordant results, but which would *a priori* be likely to yield the most accurate estimate of the expansion, was that of floating a solid sphere of iron in liquid iron of the same composition. The metal used was ordinary grey pig, and was contained in a large pot and brought to a temperature near its freezing point and spheres of metal dropped in. They were found to sink at once when dropped in cold, and they remained under the metal till they had acquired a temperature just approaching visible red; but at that temperature they rose to the surface, and as they gained more and more heat from the liquid metal their line of flotation rose higher and higher. Sometimes, if dropped in suddenly, the spheres did not float until they had begun to melt, but this was owing to their having cemented themselves to the bottom of the pot. When dropped in cautiously, or suspended by a wire, they sank only for the space of 20 to 25 seconds, and rose to the surface when barely red hot. The spheres were allowed to remain in the liquid till they began to melt, and then withdrawn and cooled, when a well-defined mark of the line of flotation was seen round the sphere. The spheres and their flotation segments were measured by several methods—1st, by callipers; 2d, photographed, and the photograph measured by a dividing engine; and 3d, by a telescope and micrometer. The last method yielding the most concordant results. The spheres from

several of the most successful experiments were measured with the following results. The numbers are merely scale readings, the spheres being 4·66 diameter.

No. I.

Diameter of Sphere.	Height of Segment.	Diameter of Segment.
2200	312	1585
2213	318	1583
2212	320	1597
2214	321	1592
2210	317	1598
2218	322	1590
2204	324	1588
2207	315	1589
2211	314	1592
2203	317	1594
Average 2209	318	1591

The variations in the measurements are due principally to the roughness of the surface after the sphere has been immersed in the liquid metal. Measurements of similar spheres gave as follows :—

No. II.

Diameter of Sphere.	Height of Segment.	Diameter of Segment.
2205	319	1584
2193	320	1591
2221	314	1596
2207	313	...
2226	317	...
...	320	...
Average 2210	317	1590

No. III.

Diameter of Sphere.	Height of Segment.	Diameter of Segment.
2221	317	1580
2204	321	1598
2207	314	1607
2200	315	...
Average 2209	316	1595

Calculated by the two methods—

$$(3r^2 + h^2)\cdot 5236h, \text{ and}$$
$$(3d - 2h)\cdot 5236h^2,$$

where r = radius of segment
h = height ,,
d = diameter of sphere.

The first method always yields the highest results owing to the diameter of the segment appearing larger than it really is; this being caused by the ridge of metal and scum which marks its flotation line. The difference between the two is not very great, but we take the value given by the last formula as the correct one. The amount by this method is 5·62 per cent. of expansion. Further experiments were tried by heating balls of iron to various temperatures and immersing them in the liquid iron to find at what temperature they ceased to sink; but this method fails for two reasons,—1st, the iron freezes a quantity of metal on its exterior, thus increasing its volume; and 2d, it gains heat so rapidly that before equilibrium is established its temperature has risen several hundreds of degrees.

The expansion obtained by the above method is rather under the truth, because, although the sphere is just at its melting-point, the liquid iron is of necessity considerably above it, so that it is not at its maximum density, which appeared to be very little if any above the melting point.

We find, then, that liquid cast iron expands at least 5·62 per cent. of its volume on freezing.

2. Researches on Contact Electricity. By C. G. Knott, Sc.D. Communicated by Professor Tait.

(Abstract.)

In these experiments the general method pursued was by direct contact and separation of two circular plane metal disks, the lower one of which was insulated and connected to one pair of quadrants of a Thomson quadrant electrometer. The upper disk or plate of this condenser arrangement pressed during contact on the lower by its own weight, and was in connection with the other pair of quadrants and with the earth. The lower plate formed the upper surface of a cylindrical flask, whose temperature was determined by

that of the water contained within it. In this way contact experiments with surfaces at different temperatures were made and results obtained. In the method which gave most reliable results, the upper surface was kept at the temperature of the air, while the temperature of the lower was allowed to vary as the water contained in the metal flask cooled through time. The contact, effected by lowering the upper plate upon the lower from a height of 5 inches, was instantaneous, so that the temperature of the upper surface did not change during the operation; while *immediately* before every such contact, both surfaces were carefully polished with emery paper and dusted, and their temperatures carefully observed. The best results were obtained when both the surfaces were of the same metal, as, for example, iron against iron. When that was the case there was, of course, no electrification by contact and separation when both plates were at the same temperature. When, however, the temperature of the lower surface was raised, a deflection on the electrometer scale was obtained, indicating a difference of electric potential at the surface of separation of these metal plates. Thus it was found that iron hot was negative to iron cold, copper hot negative to copper cold, zinc hot negative to zinc cold, and the same seemed to hold for tin. Not only so, however, but the difference of potential between, say, the two iron plates increased apparently with the difference of temperature between them, and increased *uniformly*. Curves were drawn out representing the variation of this potential difference with the temperature of the lower plate; and the points entered clustered approximately round three straight lines representing the temperature-variations for iron, copper, and zinc respectively. The tangents of the angles of inclination of these lines to the temperature axis are given in the following table:—

Metal.	Tangent of Inclination.
Copper, ...	·39
Iron, ...	·76
Zinc, ...	·9

Now it was proved by experiment in every case that this "negative-growth" of the metal surface when its temperature was raised was a *permanent* surface condition after the surface was cooled down to the same temperature as its fellow. Hence it follows that the main effect is not due to *mere* change of temperature, but to some

material alteration of the surface produced by this change of temperature—oxidation, for example. That this is the most probable view is borne out by known facts, and by certain results which I myself obtained relating to the subject of contact electricity. Hankel long ago showed that, with the great majority of metals, there was a negative-growth in time—for example, iron recently polished or filed was electrically positive to iron which had been left for some time in the air. This was probably due to oxidation; and it became a question of interest to compare this "time-variation" with the "temperature-variation" discussed above. A series of experiments were made very similar to those described above and differing only in this, that the lower surface was permitted to vary through time, without any alteration in temperature. The curves obtained by plotting the electrometer deflections against the time were very similar to the ordinary curves of cooling—somewhat logarithmic in appearance; and markedly dissimilar to the curves showing the "temperature-variation" of the same metals. Further, that metal varied in time fastest, which was the most positive: aluminium, zinc, iron, and copper being their order, taking first that one whose curve of time-variation was steepest. This result accords well with the theory given by J. Brown, Esq., of Belfast, in the "Philosophical Magazine" (1878–79), to the effect that the position of the metals in Volta's contact series depends mainly, if not entirely, upon their chemical affinity for air, the most positive being that which has the greatest affinity. That the most positive (aluminium, namely) should also be that which varies fastest in time is extremely probable; and it is also a plausible enough hypothesis that the most positive should also have the most rapid negative-growth with temperature. Now, as far as these experiments go, this is really the case. Zinc, iron, copper, are in the order of magnitude for temperature-variation, and also for time-variation; and the same order holds in Volta's contact series beginning with the most positive. It would thus appear that for any one of the metals zinc, iron, copper, and (we may add) tin, there corresponds a definite surface condition to every temperature—a condition which is permanent even after the surface has cooled, which has the effect of making the surface electrically negative to its original self, and which no amount of polishing can alter as long as the temperature is kept constant. Hence we

conclude that a chemically pure surface of these metals is impossible for more than a very few seconds after cleaning, even if for so long.

These experiments are to be repeated with the aid of an improved method of effecting contact.

3. On an Instrument for detecting Coal-Gas in Mines. By Professor George Forbes.

In 1877, shortly after the disastrous colliery explosion in the Blantyre pit, in which hundreds of lives were lost, Mr James Young, F.R.S., of Kelly, described to me an instrument which he had thought of for determining what is the amount of fire-damp in any part of a mine. This was the first thing which directed my attention to the subject, and I very soon saw that there was a principle in acoustics which might be most admirably adapted to the end in view, viz., to determine the quantity of fire-damp (or marsh gas) by the diminution in density of the mixed air and gas (for marsh gas is only about half the density of air). Mr Young and myself tested the principle the next day, and found it to be one of extreme delicacy. I then, in the spring of 1878, communicated to this Society the principle which I proposed to utilise in the form of a preliminary note. I have now the honour of exhibiting the instrument, which has been completed and perfected, partly by my own labours and partly by the appointment, for the purpose, of a Committee of the British Association, consisting of Professor W. J. Adams, Professor Ayrton, and myself. The form of instrument finally adopted is one in which a tuning-fork is set into vibration by drawing through between the prongs a tight-fitting piece of metal. Just under the points of the prongs a tube $1\frac{1}{4}$ inch diameter is fixed. The lower end of this tube is closed by a tight-fitting piston, whose position in the tube can be altered so as to regulate the length of the closed tube.

It is a well-known principle in acoustics that when a vibrating tuning-fork is so held over a tube, the air in the tube will resound and intensify the sound when the tube has a certain definite length. Moreover, this length depends on the kind of air or gas with which the tube is filled, being longer for a heavy gas, and shorter for a light gas, at the same pressure. Now a mixture of air and marsh gas is lighter than pure air in proportion as the dilution with marsh gas is

increased. Thus, according as there is a large or small percentage of fire-damp in a mine, so will the length of tube which best resounds to the tuning-fork be great or small.

There are one or two small practical details which have given some trouble, but which now render the instrument very perfect.

1. In order to give to the hand great control over the lengthening and shortening of the tube, a rack has been attached to the piston, which works in a pinion on whose axis there is a large disc with a milled head 3 inches in diameter. This disc has a glass face with a graduated scale round the circumference; so that a fixed index marks with great precision the exact length of the tube. The scale is thus made so large that readings can be made in the feeblest light.

2. In order that the instrument may be taken in advance of a lamp in places where gas is expected in large quantities, a phosphorescent powder is placed in a cavity behind the graduated glass plate, by which means readings can be taken in the dark.

3. To be sure that the gas or air in the tube is the same as what is to be found in the particular part of the mine under examination, I have introduced, through the piston which works the pinion, a rod at the upper end of which is a packed disc fitting the tube tightly. Previous to taking a reading this disc is, by means of a handle attached to the rod, driven up to the open end of the tube, and in being drawn back it sucks in the air from the place under observation. It is thus, by a single turn of the handle, locked to the piston which works the pinion, by a bayonet joint.

4. The temperature in a mine is generally very constant. But to prevent errors arising from variations in the temperature a thermometer is attached whose graduations are given in *percentages of fire-damp*, which are to be subtracted from the percentages recorded in the circular scale.

5. To test the accuracy of the scale, I have a circular trough 4 feet diameter and 3 inches deep. This is partially filled with water, and a grating is placed in the water to stand upon. In the centre of the trough there is a hole with an inch metal tube projecting upwards 4 inches. To this is attached an india-rubber tube 2 feet long, with a mouthpiece which can be firmly attached to the mouth for breathing. Sitting on the stool with the mouthpiece attached, and the nose closed by spring pincers, a tin cover 4 feet high and 3

feet diameter is lowered over me into the water. There is a small hole in this cover, with glass over it, at which a light is held on the outside. Different quantities of marsh gas are then admitted under the cover, which I mix with the air by means of a fan. A reading is taken with the instrument, and at the same time a bottle of water is emptied and closed air-tight. The contents of the bottle are afterwards analysed; and thus we obtain the true percentages corresponding to different readings of the scale.

In this way I find it possible to measure the proportion of fire-damp to about $\frac{1}{2}$ per cent.

I have taken the instrument down several fiery mines, both in Yorkshire and Lanarkshire, and have found it most accurate and consistent in its indications. Messrs Merry & Cunninghame, after trying it, have adopted it. It is extremely portable, can be carried in a large coat-pocket, and is not likely to be injured, and causes no trouble. In fact, in this, its latest form, it seems to answer all requirements. Variations in the pressure of the air do not affect it.

I ought to add that although choke-damp (*i.e.*, carbonic acid gas) is not often found in company with fire-damp, yet even when this is the case, and in sufficient quantities to prevent the instrument from indicating the presence of fire-damp (choke-damp being as much heavier than common air as fire-damp is lighter), its presence prevents the fire-damp from being explosive; and thus the indications of the instrument can in all cases be relied upon for indicating danger.

4. On Comets. By Professor Tait.

(*Abstract.*)

The author commenced by stating that he had been led to make farther investigations, on the subject of his hypothesis as to the nature of comets, by some comparatively recent criticism to which that hypothesis had been subjected. Its main features had been published more than ten years ago in the "Proceedings" of the Society (May 17, 1869) and in the first volume of "Nature." Of course, if a critic completely misstates an hypothesis, he has no difficulty in refuting it; so that to such writers the author does not attempt to reply. The other class of critics, including Mr Glaisher, and the late

Prof. Clerk-Maxwell, while on the whole favourable to the theory, pointed out the necessity for a full dynamical investigation, whose results might be compared with observation. The author's own conviction has all along been that the difficulty is not so much dynamical as constructional:—*i.e.*, it lies mainly in obtaining a proper conception of the problem to be treated in the case of any particular comet, and not in the way of obtaining at least an approximate solution when once the problem is stated. The fact is that the hypothesis is so very general that almost anything could be explained by it. When two considerable masses of stone, moving approximately in the same orbit, impinge on one another with given velocities, what is the amount of smashing—how many large fragments, how many small, how much mere dust, will be produced—and in what direction and with what relative velocity will each of these on the average be projected? What amount of glowing gas will be produced? Again, if there be many millions of such masses, forming a group in which all describe approximately elliptic orbits in something like equal periods, but of various sizes and in any planes about their common centre of inertia, the group itself being subject to a sort of tidal disturbance by the sun, at what part of the group will the impacts mainly occur? Questions so entirely vague as these are not yet ready for the application of mathematical methods.

The main difficulty felt by the critics above named seems to be with respect to the production of the *tail* of a comet. The hypothesis of course involves as an immediate consequence that extensive regions of space all round the nucleus of the comet (but specially extended in the plane of its orbit) are full of fragments large and small, driven out at different times from the main ranks which (on the whole) become gradually extended along an arc of the orbit. Rays or tails will thus be seen wherever a visual line can be drawn, along and near to which there is an assemblage of particles fitted to give back a maximum of solar light. And, if the particles be not very large, the mass in each cubic mile of space may be very small, while the whole has considerable brightness, and yet does not sensibly weaken the light of a star seen through it.

The author stated that he had investigated the form assumed by a train of particles ejected at different times from the head of a comet in the plane of its orbit; always with the same relative velocity (so small

that the square of its ratio to that of the comet may be neglected), and in a direction making a given angle with the tangent to the orbit. The result is that such particles will lie approximately on a semi-parabola, the vertex being at the head of the comet. When the ejection is towards regions outside the orbit, the parabola lies behind the head of the comet; but if the ejection be inwards the parabola precedes the head. This parabola diminishes in parameter as the curvature of the orbit increases.* There can be no doubt that here we have a very striking resemblance, if no more, to the form usually assumed by the tails of comets; and for comets with many tails (like that of 1744) we require only a greater number of definite directions of maximum ejection. *Why* this ejection is generally (though by no means always) outward, (for several comets have had two tails, of which one was turned *towards* the sun) we cannot attempt to explain till we know at what part of the group of masses the impacts are most likely to take place.

The present theory differs altogether from that of Olbers, Bessel, and others, in assuming the fragments which form the tail to have but little velocity relatively to the nucleus, while the received theory assigns them very rapid motion along the tail:—Olbers says as much as a million miles per day. The one theory endeavours to represent the motion as a result of the received law of gravity; the other introduces the hypothesis of a solar repulsive force often

* These conclusions are found to follow easily from the very simple investigation for a circular orbit. For the approximate differences of radius-vector, and angle-vector, at the time t, of the comet and of a particle projected at time t_1, with relative velocity p, from its head, in a direction making an angle ψ with the tangent, are—

$$r-a=-\frac{p}{\omega}\left(2\cos\psi\,(1-\cos\omega(t-t_1))-\sin\psi\sin\omega(t-t_1)\right),$$

and

$$\theta-\omega t=\frac{p}{a\omega}\left(\cos\psi\,(3\omega(t-t_1)-4\sin\omega\,(t-t_1))+2\sin\psi\,(\cos\omega(t-t_1)-1)\right).$$

Here a is the radius of the orbit, and ω the angular velocity in it.

If $\omega\,(t-t_1)$ be a small angle χ, whose third and higher powers may be neglected, these expressions take the form—

$$r-a=-\frac{p}{\omega}\left(\chi^2\cos\psi-\chi\sin\psi\right),$$

$$\theta-\omega t=-\frac{p}{a\omega}\left(\chi^2\sin\psi+\chi\cos\psi\right),$$

from which we easily deduce the results stated above. It appears that in the majority of large comets ψ is nearly a right angle.

more intense than gravity, and one which, unlike gravity, depends on the quality as well as the quantity of matter. It seems to the author that the introduction of such hypotheses is inconsistent with Newton's "Regulæ Philosophandi" until it is definitely proved (as has certainly not yet been done) that known forces are not competent to produce the observed results.

5. Additional Observations on Fungus Disease of Salmon and other Fish. By A. B. Stirling, Assistant-Curator in the Museum of Anatomy in the University of Edinburgh. Communicated by Prof. Turner.

In a paper read to the Society in June last, "Proceedings," June 1879, on the fungus disease affecting salmon and other fish, I discussed the various theories which were advocated, as to the cause of the disease; the effects of the fungus upon the fish, the vegetative and reproductive aspects of the fungus, and the belief that salt water had a curative effect upon salmon affected with fungus disease, on their reaching and remaining for some time in that element.

I also mentioned that at the instance of the Tweed Commissioners, an experiment was being conducted by G. H. List, Esq., to test whether that belief was well founded. I will now state the nature and result of the experiment, and afterwards relate to the Society, an account of an epidemic of fungus, which occurred at Ightham Mote, in the county of Kent in 1874, and which appeared again in a virulent form a few weeks ago.

The experiment referred to was conducted as follows:—A wooden cage, large enough to allow a salmon to move about within it, and perforated with holes so as to allow the water to flow freely through it, was prepared. It was then moored in the tideway in the River Tweed, below Berwick bridge, where the water is at all times more or less salt, and was now ready to receive a fish for experiment.

About the end of May last a sea-trout kelt was captured at Eithermouth, three miles up the river, and within the influence of the tide. It was placed in a suitable vessel and conveyed to Berwick, where it was enclosed in the cage. The fish is stated by Mr List to have been affected with a sloughing sore on the top of the head from the point of the snout about two inches in length and

the same in breadth. This sore had all the characters of a sore produced by the fungus disease.

The cage was visited at intervals, and the effects of the salt water upon the fish noted. The cage with the fish was towed out to sea for two hours on each of six occasions.

In a short time, the sore upon the fish was observed to be healing. The fish remained in the cage till the 2d or 3d of October, when an accident occurred by the breaking of one of the chains which held the cage in position. This allowed the cage to swing to one side, and nearer to the shore, when upon the ebbing of the tide the cage was left dry, which occasioned the death of the fish. On discovery of the accident, the fish was sent to me for examination. I received it on 4th October, after it had been confined in the cage for fully four months. From the combined effects of imprisonment, and want of food, it had become very much shrunken, was very lean, and had more the appearance of a compressed eel than the form of a salmon. The fish was very dark in colour, the scales were uninjured, and the mucus covering was evenly thin and transparent, and there was no fungus on any part of its body. All the viscera were healthy the sores upon the head were healed, and the skin grown over them. A slight sore on the under surface of the right lower jaw, which appeared to have been caused by friction on the bottom or sides of the cage, was in a raw condition, but had not the least appearance of an unhealed ulcer. Fully one-third of the lower border of the upper lip, at the middle of the snout, and the outer and upper margin of the gums at the same part, were not quite healed, and the roots of the teeth were exposed from the parts having been ulcerated.

The pectoral and caudal fins had been diseased, and some of their rays broken; both were now healed and covered with membrane, and the shortened rays had the appearance of growing again. I consider the result of the experiment to be so far satisfactory; it shows that migratory *Salmonidæ* affected with an ulcer produced by fungus disease, get rid of it in salt water, even when confined and without food for a long period, and I infer from those facts, that had the fish experimented upon been free in the ocean for an equal period of time, it would have recovered both health and condition.

The removal of all dead fish from the rivers has been universally

advocated; the removal and killing of all affected fish has been recommended by many. On the other hand, the Tweed conservancy hold the opinion that the capture and removal of all fish affected with fungus (not in a dying state) to salt or tidal water was the proper course to follow, and with this opinion I fully concur. There is one point in this plan which may cause some disappointment. Supposing it proved that salmon are cured of fungus disease in the salt water, and that those so affected in the upper waters, were captured and conveyed to the tidal part of the river, only those fish with the instinct of descending to the sea, when captured, would remain to be cured. Those with the instinctive desire to ascend, when captured, would in all probability return to the fresh water. Those instincts in the salmon are known to be both strong and certain, their sense of being diseased, and need of cure "instinctively or otherwise" are unknown.

I shall now give an account of the very remarkable epidemics which occured at Ightham in Kent, the particulars of which were kindly communicated to me by Dr W. S. Church, Physician to St Bartholomew's Hospital, London. They are of so much interest in the history of the fungus disease, that I feel warranted in bringing them to the notice of the Society. Ightham House dates from the time of King John, and the fish ponds were probably constructed at the same time, to supply the house with fish. The house is built in the form of a square, and surrounds a courtyard. The house in its turn is surrounded on all sides by a moat, the water in which is from 5 to 9 feet in depth. The present arrangement of the ponds, garden, &c., was probably made in the time of James I. The house drains into the moat, and the drains issue into it by separate openings from two sides of the square. The stream which supplies the ponds and moat is formed by the surface water of a small valley, but is principally supplied by two very fine and strong springs, which come out of the Kentish limestone. The stream is only about a mile in length before it enters the upper pond, and there is at all times a strong run of water in it. It is perfectly free from drainage contamination, and enters the upper pond perfectly pure. There are two cottages and a small fold yard on the side of the stream, but no drains flow from them to the water; the fold is in a ruinous condition, and is not in use.

The ponds are much larger than the square of the house and moat. The upper pond is situated about 100 yards above the moat, the greater part of the space between them being occupied by a bowling green. This pond is shallow, and has reedy banks, with flags and aquatic plants growing on the margins; a strong current flows constantly through it to the outlet, and the water leaves it by a stone channel falling perpendicularly about 5 feet.

Immediately beyond the fall, the water divides and forms two open streams, which supply two small ponds or stews at a short distance below, on the right and left sides of the fall. The water leaves the stews by conduits, which pass underground to the moat, and enter it by two falls of between 3 and 4 feet each, which fall clear of the breastwork. In addition to the main stream, through the conduits there are two other strong feeders of the moat, which flow into it from springs on opposite sides, and there is a continuous current flowing through it. The water leaves the moat by culverts to the lower pond, and from the lower pond by a fall, and flows through grass fields for a mile, where it enters another fish pond.

No epidemic of fever or other zymotic disease is known to have taken place in the house, and only two cases of sickness (measles) during the last fifteen years. The gardener, his wife, and child, were the only occupants of the house during last winter, spring, and summer, the family being from home.

Several severe epidemics of fungus have been observed in the ponds and moat. One occurred about the year 1850, but no particulars have been preserved, and mild ones may have passed without much notice. "Furred" fish, and even a few dead ones, have been often seen by the gardener. In the spring of 1874 a very severe epidemic occurred, when all the ponds and the moat suffered heavily; nearly all the fish died in the moat, and the disease was very destructive in both the upper and lower ponds.

This attack was inquired into by Dr Church, who satisfied himself that the fungus affecting the fish was *Saprolegnia ferax*. The fish consisted chiefly of roach, pike, and dace in the moat; roach, perch, and pike in the upper pond; roach, dace, perch, pike, and gudgeon in the lower pond. The roach, dace, and gudgeon suffered the most, only the small pike and perch were affected, and none of the large pike or perch were found dead, and not a single eel.

Many of the fish looked, when in the water, as if covered with a halo, remaining at the surface nearly motionless, frequently putting their mouths out of the water, and turning belly uppermost immediately before death. On examination, the fungus was found to be most thickly matted on the shoulders just behind the head, clogging up the gill openings, on the pectoral fins, and tail portion of the body. Whenever ulceration had taken place, it was seen to be due to the fungus, as the parts most ulcerated were those most densely covered with fungus. Death was caused by suffocation in every instance.

The last fungus epidemic which occured at Ightham moat and ponds began in the latter end of October of the present year, and continued to the middle of November. About eight or ten days after it had commenced, and numbers of the fish were observed to be dying, Dr Church very kindly favoured me by sending a number of specimens that had died in the water, and also a number that were affected with the fungus but were still alive when taken from the water. Dr Church informs me that in this epidemic it was chiefly the fish in the moat which were affected and died, and only a few in the lower pond were observed to be affected; none were found affected in the stews and upper pond, although the stews were swarming with fish. As during the epidemic of 1874, the roach and dace suffered first and worst; the pike, perch, and eels have not been affected during this epidemic.

The diseased fish sent to me by Dr Church were roach, 17 in number; 7 were dead when taken out of the water, and 10 were alive when taken. They average 2 oz. in weight each, and were all packed in fresh grass; those taken alive were put at the bottom of the box, with grass under and over them, and the others at the top of the box were packed in a similar way. The fish at the top of the box were overlying each other, and appeared as if they were enclosed in a common envelope of fungus, and such was actually the case; the fungus having continued to grow vegetatively, had, as it were, woven the whole group in a web of fungus. The new growth had a perceptible pink tint, the same as I had seen upon a greyling sent to me from the river Tweed last spring, and may possibly be the natural colour of the fungus when it grows in the air. I confirm Dr Church's statement that the fungus was *S. ferax* and identical with

that found upon diseased salmon from the Tweed and Solway rivers during the epidemics of 1878 and 1879. I observed that the majority of the filaments of the fungus found upon the Ightham fish were spear-shaped, very few had clavate fruit heads, and I saw none with ripe zoospores, indicating that the reproductive power of the fungus was feeble, and was producing only barren filaments, which appears to be always the case when the epidemic has run its course.

Only four of the ten specimens had any external blemish upon them, which consisted of slight ulceration upon one pectoral fin in two, and in the caudal fins of other two; several of the rays were broken, and portions of them were hanging by the filaments of the fungus. In all the specimens the fungus covered the greater part of their bodies; and the heads of several, including the eyes and nostrils, were completely covered over. In none of the fish were the gills affected, but five of them had the opercular opening of the gills nearly closed up by the fungus.

On opening the abdomen the viscera were seen to be white, firm in position, and with a fair amount of fat upon the stomach and intestines. The roe in the females was firm and clear, and though very small, it was more advanced then the milt in the males. The heart, liver, and spleen were normal in size, and not the least appearance of extravasation in any of the organs. On opening the stomach and intestines I could not determine what the food of the fish had been, as only white glairy mucus in small quantity was found in any of them.

The blood was perfectly normal in all, and the subcutaneous tissue was in no instance discoloured, even under the thickest patches of fungus, showing that up to the time the fish were captured no ulceration, or indication of any, either on the head or scaled parts of the body, had taken place. The seven specimens preserved and submitted to the Society will be found to be without a sore or an ulcer on any part of them, which *S. ferax* could claim as a pre-existing nidus upon which to plant itself. I may notice here that there seems to be two ways by which the fungus causes death by suffocation. The first and quickest way is when the fungus gets seated within the mouth and upon the gills at the same time, which I have observed occurs oftener in the large fish than in the small. The second, and

probably a slower way, is when the fungus grows over and closes up the opercular openings of the gills, which seems to be the way those specimens have been suffocated, being shrouded while alive in fatal fungus, they have died in their beauty, with their silvery skins unbroken.

There is one fact connected with the Ightham epidemic, namely, that the large pike, perch, and eels were not affected by the fungus disease. I am unable to account for this immunity on physiological principles, and refer it to the hypothesis of the "struggle for existence and survival of the fittest." It would be difficult to find anywhere a purer collection of water than the Ightham ponds. The main stream, upper pond, and stews, being virgin spring water, uncontaminated with any pollution, so that I am convinced that *S. ferax* can and does exist where no source of pollution is present, and exercises its destructive influence upon the fish as is evidenced by the numerous deaths in the epidemic of 1874 in the Ightham upper pond. Up to the present time, it has generally been held that fungus epidemic, or, as it has been called, salmon disease, was con fined to and had its origin in rivers frequented by the migratory *Salmonidæ*. At an early stage of the inquiry, Sir Robert Christison referred to this, and urged that if possible it should be ascertained whether the disease had ever been observed in the head waters of any salmon river above any impassable obstruction, either natural or artificial. The epidemic at Ightham moat and ponds answers the question Sir Robert desired to be cleared up, and proves that *S. ferax* is not confined to rivers frequented by salmon.

In a former paper, I stated that the so-called salmon disease did not depend upon a pre-existing functional disorder in the fish. I am still of this opinion, and point to the fish from Ightham as a further proof that this is the case. I also stated my belief that *S. ferax* existed at all times and probably in all waters, and that the presence of fish and *S. ferax* in the same water under certain climatic or other at present unknown influence, seems all that is necessary to originate fungus epidemic.

The epidemic at Ightham in my opinion does away with the theories of overcrowding, including overstocking. Overcrowding of salmon in a pool in a river is not analagous to overcrowding of people in a room or in a prison cell, where only a certain amount of

air can circulate. Salmon crowded in a pool in a river, through which a stream of water flows freely, are in a condition similar to a herd of cattle crowded in a pen or fold, in the open air on a hill-side, where pure air is inexhaustible. In like manner salmon crowded in a pool are provided with a continuous supply of oxygen by the constant flow of the river through the pool.

As to overstocking, my own opinion as an angler of fifty years' experience, and as a net fisher for a fifth of that time, is that I never found the fish too plentiful anywhere; and I do not think there ever can be too many, especially trout, grilse, and salmon, in any of our rivers. Very curiously, those who advocated the theory of overstocking as the cause of the fungus disease, are in many instances the very persons who grumble at the scarcity of the fish in question, and propose to increase their number by killing them for eight or ten days longer at the latter end of the season, when the fish best adapted for breeding are entering the rivers. Regarding the food supply in overstocking, I quote the following statement cited by Sir Samuel Wilson of Ercildoune, Australia, in his work on the acclimatisation of Californian salmon. "It is stated by Mr Vincent Cooke of the Oregon Packing Company, that out of 98,000 salmon caught in the Columbia River in 1874, three only were found with some trace of food in their stomachs, and those seemed to have quitted the salt water very recently."

The fact that the house drains into the moat might be urged by some as an argument that the water there is rendered foul by the house sewage, and that the pollution of the water may have had some influence in developing the disease. In reply to this it must be stated, that, as Dr Church points out, a large body of water flows through the moat hourly, and so far from ordinary house drainage being prejudicial to fish, where the water is frequently changed, the finest fish, as the pike, perch, and eel, are to be caught in the neighbourhood of the house drains. But if it were proved that the house drainage mingling with the water of the moat served as an exciting cause for the development and propagation of the fungus in the moat, this could not be advanced as a reason for the appearance of the disease in the upper pond, which was fed by an uncontaminated stream. Neither could diseased fish from the moat find their way to the upper pond so as to infect the fish there, as there is not only a

clear fall of between 3 and 4 feet between the moat and the stews, but one of about 5 feet between the stews and the upper pond, thus presenting obstacles such as the fish living in these waters could not surmount.

In conclusion, I feel convinced that the so-called salmon disease is *the fungus* itself, and that no structural disturbance in the fish is necessary to cause fungus attack; that this appears to me to have been abundantly proved by the sixty specimens which I have dissected and examined; that it is useless to look for more information on the origin and cause of fungus epidemic, from the carcases of salmon or other fish affected with the fungus; that the origin and cure or prevention of the plague must be sought for in the life history of the plant, which is more the work of the botanist than the anatomist.

PROCEEDINGS

OF THE

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NINETY-SEVENTH SESSION.

Monday, 5th January 1880.

The Right Hon. LORD MONCREIFF, President, in the Chair.

The following Communications were read:—

1. The Trigonometrical Survey of Palestine. By Lieut. Claude Reignier Conder, R.E.

The survey which forms the subject of the present paper extends over an area of 6000 square miles, bounded by the Jordan on the east, the Mediterranean on the west, the river Leontes and the springs of Jordan near Dan on the north, and the desert of Beersheba on the south. Within these limits a complete triangulation, with two bases each about four miles long, has been extended, and the whole of the country mapped to the scale of one inch to the mile. The work occupied five years in the field, and nearly three years more in preparing the results for publication.

The trigonometrical survey was first commenced in October 1871, when a party consisting of only four Europeans took the field. This expedition was sent out by the Palestine Exploration Fund, under the auspices of which society, Captain Warren, R.E., had previously been employed in his adventurous explorations at Jerusalem during the years 1867 to 1870.

The officer selected for the command of the survey party was Captain Stewart, R.E., of the Ordnance Survey. Unfortunately the

expedition reached Palestine in the most unhealthy season of the year, and their leader was invalided home in January 1872. The two surveyors, Sergeant Black, R.E., and Corporal Armstrong, R.E., were thus left under the care of Mr. C. F. Tyrwhitt Drake, who had been attached to the expedition as linguist and archæologist.

On the return of Captain Stewart, the Committee of the Palestine Exploration Fund honoured me with the offer of the command of the survey party, and I reached Jaffa on the 8th of July 1872. In the meantime the survey had been successfully started, and after measuring the first base in the plain of Sharon near Ramleh, the triangulation was extended first to Jerusalem and then northwards to Nablus or Shechem, the detail being at the same time filled in over an area of 500 square miles.

The success of this part of the work was due not only to the zeal and skill of the two non-commissioned officers, but also in a great measure to the tact and experience of Mr Tyrwhitt Drake, whose knowledge of Syrian manners and language was invaluable.

The method of conducting the survey was soon developed into a routine which was preserved throughout the course of the field operations. The camp having been fixed in a convenient position and as centrally as possible, with reference to the proposed work, the triangulation was first extended. The highest points within a radius of ten or twelve miles from camp, were visited, and at the points selected cairns of stone seven to ten feet high were built up and carefully whitewashed. In some cases the domes of the sacred tombs formed valuable stations, and in the more wooded parts of the country it was necessary to clear away the brushwood, leaving a lofty stack of branches bound to a central pole marking the instrumental station.

The triangles thus constructed varied from five to fifteen miles side according to the character of the country. Several very long lines were also observed, and from the ends of the bases astronomical observations were taken to fix the meridian lines. The observations as calculated at Southampton in 1877 showed an error rarely exceeding thirty feet in the mile, which it is unnecessary to remark is not visible on paper to the one-inch scale.

From the trigonometrical stations observations were also taken to all prominent objects within the field. It was found, however,

that the effects of mirage rendered these observations untrustworthy at a distance of more than about six miles from the station. The sacred tombs, solitary trees, village towers, and other conspicuous landmarks were fixed by the intersection of the various angular directions thus observed, and these served as secondary points in the work which next followed.

The trigonometrical observations were calculated in camp and laid down on rough sheets. Tracings were then prepared for each of the surveyors, and the district surrounding the camp subdivided. Each surveyor, accompanied by a local guide, then proceeded to fill in the detail of the sheets with the aid of the prismatic compass and the names of the various features were carefully collected from the guides, and verified as far as possible by reference to independent witnesses.

The detail which was shown may be seen on the lithographed sheets. It includes towns and villages, ruined sites and isolated buildings, springs, wells, cisterns and aqueducts, enclosures and roads, all the principal isolated trees, and the cultivation or natural growth of the country, rock-cut tombs, vineyard towers, wine presses, and other traces of ancient cultivation, as well as the dry torrent beds and perennial streams which form the natural, boundaries of the ancient divisions of Palestine.

The collection of the names was one of the most delicate and important parts of the work. It is well known that the nomenclature of Palestine, so far at least as the sites of towns and villages are concerned, has remained almost unchanged from a very remote period.

The reasons for this preservation of the Hebrew nomenclatnre will be mentioned later; but it may be noted that one of the original objects for which the survey was undertaken, was that of collecting ancient names hitherto unknown, for the purpose of assisting in the identification of biblical sites, especially in those districts of the country previously almost unexplored.

In order to secure the correct orthography of the nomenclature a native scribe was attached to the party, and in order to secure the correct application of the names it was made a rule only to inquire on the very spot. The site being thus ascertained, the correct pronunciation was obtained on the evening of the same day from the local guide on the return of the surveyor, and was written down by the scribe. 10,000 names were thus collected in 6000 square

miles, and although a large proportion of these are of little value, the result of this systematic examination of the nomenclature has been the recovery of about 150 ancient sites, which are newly identified with places mentioned in the Bible, while a yet larger number of names connected with the Byzantine and Crusading history of Palestine have also been recovered.

The elevations of the trigonometrical stations above the sea were obtained by vertical angles with the theodolite, and checked by means of measured heights along the coast. The error in these heights does not appear to exceed three or four feet on an average.

Other elevations were observed with aneroid barometers, and checked by readings taken at bench marks or at trigonometrical stations and by readings of the mercurial barometer in camp. A line of levels had been run from Jaffa to the Dead Sea in 1864 by Captain Wilson, R.E., and with this our trigonometrical observations agreed very well. The summer level of the Dead Sea was thus fixed at 1292·5 feet below the Mediterranean, the surface in summer being about fifteen feet lower than in winter.

In 1875 a second line was run by the survey party from the Bay of Acre to the Sea of Galilee, a distance of about thirty miles, and the level of this lake was thus fixed at 682·5 feet below the Mediterranean. This piece of work was carried out under a special grant from the British Association, former estimates of the depression having ranged from 300 to 600 feet.

By means of these levels and of observations to trigonometrical stations between the two lakes and farther north, the fall of the Jordan was determined throughout the whole of its course.

The rate at which the survey was carried on increased gradually as the party became more accustomed to the country and better acquainted with the language and customs of the natives. At first it did not exceed fifty square miles in a month, but after leaving Nablus an average of 100 was obtained. In 1873 this was increased to 150, and in 1874 to upwards of 200. The party was strengthened in the latter year, when an average of 270 square miles per month was attained, and kept up almost to the end of the survey. The most rapid piece of work was the survey of the Desert of Judah. The triangulation being very large and the detail less close than in the cultivated districts, the survey of this desert was completed in ten

days, the area being 330 square miles of very difficult mountain country.

The representation of the hill features was of necessity less exact than might be possible on the one-inch scale, but so far as the ridges and prominent features are concerned it is supposed to be accurate. The general slopes of the mountain sides were determined with a pocket level, and separate sheets of hill shading were prepared and carefully inked in before leaving the camp for a new district.

The preceding notes will, it is hoped, give some idea of the character of the survey work. The rapid rate of progress had many advantages, amongst which was the economical character of the work, the map costing in the field not more than one penny per acre of ground surveyed.

In addition to the map work many other kinds of information were supplied. The ruins were explored, plans of all interesting places, special surveys of towns and ruined cities, and notes on the archæology of the land, were regularly collected. The geological structure of the country was observed, collections of fossils and of lithological specimens were made, birds were shot and stuffed, notes on the traditions, manners, and language of the peasantry were recorded. Photographs of interesting places were also made, and a regular series of meteorological observations was kept for three years.

It was also necessary to study carefully the literature of the subject, in order that intelligent explorations might be attempted, and special observations taken in connection with the numerous controverted sites of importance throughout the country. The winter seasons were employed in reducing to order the field observations, and in preparing, by study of the literature, for the examination of questions connected with the topography of the district next to be surveyed.

The results of these explorations are not as yet fully published. The lithographed sheets now on the table have been prepared at the Ordnance Survey Office, Southampton, from MSS. worked out in England after the return of the survey party. An engraved map to a smaller scale ($\frac{3}{8}$ inch per mile) is also in course of preparation, and the proofs seem to promise that the finished work will be of excellent execution.

A memoir has also been prepared by the survey officers to accompany each of the 26 sheets of the one-inch map, and this great collection of notes is now going through the press under the editorship of Col. Wilson, R.E., whose absence on special duty in Anatolia has, however, unfortunately delayed the publication.

The memoir is divided into four sections. The first descriptive of the country, its natural features, its towns and villages, with special notes on the ancient history of the various sites. The second section deals with the archæology of the sheet, every ruin being described, with accompanying plans and sketches. The third section consists of translated name indexes, giving the Arabic lettering and the connection of ancient with modern names. The fourth section includes notes on the ethnology of the sheets. Special papers on the physical geography, architecture, nomenclature, and archæology of the various districts, on the geology of the country and on the ethnological peculiarities of the natives, will, it is hoped, be added, and the whole memoir will probably exceed 1000 pages quarto of print, with more than 100 special plans and surveys.

Some account may now be desirable of the history of the undertaking, which was not free from vicissitudes, and of the most interesting results of the exploration apart from the survey map itself.

Leaving the well-watered vale of Shechem on the 16th August 1872, the survey party proceeded northwards, and in September encamped at Jenin on the south edge of the great plain of Jezreel or Esdraelon—a plateau divided from the Jordan valley by the ridge of Gilboa, and from the plain of Sharon by the spur which runs north-west to the Carmel promontory. In this plain the second or check base was measured, and a new triangulation extended from it in a very satisfactory manner. The hills round Nazareth were surveyed during the autumn months, and the party wintered safely in the German colony at Haifa under the slope of Carmel.

Several instances of molestation occurred during this campaign to different members of the party, the most serious being an assault on Sergeant Black by the young men of a village, who were firing at a mark, but sent several bullets in an opposite direction, which fell at the sergeant's feet as he was taking angles. These offenders were, however, punished by fine and imprisonment.

In the early spring of 1873 the expedition turned south, and commenced to fill in the country lying between the sea-shore and the mountains forming the back bone of Palestine, which had been previously surveyed. The plain of Sharon was thus visited during the most healthy season, and a pleasant time was spent in a district previously but little known.

Among the most interesting places visited was Athlit, the ancient Castel Pelegrino—a fortress of the Templars, held by the Christians to within a few months of the fall of Acre in 1291, and one of the best preserved examples of crusading architecture in Palestine. On Carmel the remains of an unknown synagogue were explored; at Cæsarea the ancient temple built by Herod the Great in honour of Augustus was discovered though not completely examined. The identification of Antipatris with the ruins of Ras el 'Ain was confirmed by the survey operations; and the great wood called Drumos by Strabo was for the first time thoroughly explored, at the north end of the Sharon plain and at the foot of Carmel.

The party rested during the summer months on the heights of Anti-Lebanon above Damascus, and in October the ascent of Mount Hermon was accomplished and a night passed on the summit; the latitude, longitude, and elevation of the highest peak, 9200 feet above the Mediterranean, being carefully determined by trigonometrical and astronomical observations.

Leaving the Anti-Lebanon on 24th of September 1873 the party marched to Beirut, and thence down the whole coast as far as Jaffa—the distance of 220 miles being accomplished in seven days. The survey was next extended southwards from the former limits —the Judean hills round Bethlehem being carefully examined This part of the work was of great interest in consequence of the number of biblical sites included in the district. The most valuable discoveries were perhaps those of the rock Etam, in a cleft of which Samson hid from the Philistines, and of the probable site of the village Emmaus, sixty stadia from Jerusalem, at the present ruin of Khamasa, south west of the Holy City.

East of Bethlehem the desert of Judah was next entered, and the survey extended to the cliffs west of the Dead Sea. In this desert the name *Suk*, applying to a mountain where the scape goat used to be destroyed in later Jewish times, was found still surviving

at the distance from Jerusalem given in the Talmudic writings. The fatigue of the work was here much increased by the great power of the sun, in a district entirely bare of trees and composed of steep ridges of white marl with precipitous limestone gorges, the only water obtainable being warm and brackish and at times very scanty in quantity.

On the 15th of November the broad plains of Jericho were reached, and a district of about 200 square miles north of the Dead Sea was surveyed. No traces of the Cities of the Plain were, however, found, and the conclusion resulting from a careful examination of the ground was that these towns probably stood east of the Dead Sea or higher up the Jordan valley, where fresh water would be found to supply them. The plains of Jericho are deficient in water supply, and the soil is so deeply impregnated with salt that it seems impossible that it should ever have been cultivated within historic times.

At Jericho the expedition suffered severely from an epidemic of malarious fever, which so weakened the party as to render field work impossible during the winter. Mr Tyrwhitt Drake narrowly escaped with his life, and only three members of the party of twenty-five individuals remained unaffected. The winter months were passed at Jerusalem, and the weather experienced was unusually severe, seven falls of snow occurring in the hills, while the Jordan valley was flooded and rendered impassable.

In the end of February 1874 the expedition again took the field, and the most difficult and dangerous part of the survey was, during the next two months, carried through successfully.

This task consisted in the exploration of the Jordan valley between the plains of Jericho and the Sea of Galilee—a district entirely uncultivated and inhabited only by nomadic Arabs living in tents. The supplies were brought down from Jerusalem or Shechem, a distance often of two days' journey, and the party was obliged to trust entirely to itself for defence, as the Turkish government exercises but little control over the Bedawin. The work was interrupted by constant storms, and the oppression of the atmosphere at a level 1000 feet below the sea was found extremely trying. The water supply was very uncertain, and for ten days the party were obliged to rely on the salt springs of Wady Maleh. During

the last two weeks the power of the sun became so great that work was only possible in the early morning, for the party remained in valley even after the Arabs had retired to the hills. The exhaustion due to this campaign necessitated a long rest for the whole party, and the expedition experienced a sad and serious loss in the death of Mr Tyrwhitt Drake, who sank under a second attack of fever brought on by the malaria of the valley and the toils and privations of the survey work.

It was not until the 5th October that the expedition was again able to take the field. Lieutenant Kitchener, R.E., was appointed to take the place of Mr Drake as second officer for the party. At this time more than half the survey (3500 square miles) had been completed, including all the country between Bethlehem and Nazareth, and it was determined to complete the southern portion of the work, 1200 square miles, before attempting the survey of Galilee: the autumn of the year was consequently passed in the hills of Hebron and the desert of Beersheba. The season was unusually sickly, and the mortality in the plains was in some villages not less than 50 per cent. of the native peasant population. In the high mountains the party were, however, comparatively safe, although it became necessary to invalid one of the most valuable members (Sergeant Black) during the winter. An attempt was made to push the work through the desert west of the Dead Sea about Christmas time, but the expedition was driven to shelter by a succession of violent gales which nearly wrecked the camp.

In the spring of 1875 a very light expedition was organised for the exploration of the desert. The invalid members of the party, including Lieutenant Kitchener, remained in Jerusalem with the baggage, and accompanied by two corporals I set out carrying only the barest necessaries, with food and provender for two days at a time. The work was pushed on with the greatest possible rapidity, and as before stated 330 square miles were mapped in ten days. The shores of the Dead Sea, here girt with cliffs 4000 feet high, were visited, and a special survey made of the famous fortress of Masada. The Arab tribes were found in a very disturbed condition, in consequence of recent tribal conflicts, and this portion of the work was perhaps the most adventurous episode of the survey.

In the middle of March the survey of the low hills and plains of

Philistia was commenced and carried successfully to the southern limit of the map at Gaza. The site of Adullam and of the famous cave of the same name was determined for the first time, the ruins of Ascalon were examined, and the sites of Ekron, Gath, Ashdod, and Jamnia surveyed. The work was comparatively easy in this open and cultivated district, and the total area surveyed was thus quickly raised to 4500 square miles.

After a month's rest at Jerusalem the party marched northwards in July, and the survey of Lower Galilee was commenced, including the line of levels from the Mediterranean, which necessitated an encampment close to an unhealthy swamp north of Nazareth.

The expedition then moved northwards to Safed, the intention being to carry on the work until the winter in the mountains of Upper Galilee, leaving only the upper Jordan valley and the plain of Phœnicia to be completed in the spring of 1876.

Unfortunately the work was for a time completely stopped by a combination of difficulties. The party was attacked on the 10th of July by a mob of fanatics at Safed, and for a short time was in considerable danger; the prompt assistance sent by the Turkish Governor rescued us just as resistance began to become no longer possible, but scarcely a member of the expedition escaped without more or less serious injury.

It was impossible after this to carry on the work until the assailants had been punished, and the party consequently retreated to Carmel. The principal offenders were tried and imprisoned, and a fine of £270 was inflicted on the town. At this time, however, the whole expedition succumbed to fever, partly due to the injuries received, and a serious outbreak of cholera throughout Syria rendered it prudent to withdraw the party from the country.

The members of the expedition continued to suffer from fever on their return to England, and the field work was consequently suspended during 1876. In 1877, as my own health continued to be unsatisfactory, it was considered best to divide the party. Half of the expedition was sent out under Lieutenant Kitchener, to complete the 1300 square miles which remained to be surveyed; the other half was retained under my direction, to work out in London the results already obtained, representing four fifths of the whole work.

The survey of Upper Galilee was successfully and peacefully carried out by Lieutenant Kitchener, and as the population of the district was chiefly composed of Christians and Jews no further serious difficulties were encountered. The most valuable discoveries in this part of Palestine were the various cromlechs found by the survey party,—the first undoubted specimens of rude stone monuments as yet discovered west of Jordan.

The construction of a large scale map was not the only duty of the survey party. Information was also expected on all antiquarian and scientific questions which it might be possible to examine. Among these the principal results connected with the ethnology, geology, zoology, physical topography, and architecture may be briefly noticed, and a few words added in conclusion respecting some of the biblical sites discovered by the survey party.

The great explorer, Dr Robinson, was one of the first writers who called attention to the conservation of ancient names and traditions among the Syrian peasantry. The collection of 10,000 local names during the course of the survey, not only resulted in the addition of many new sites to those already known, but served to throw light on the reason of the preservation of ancient Hebrew names almost unchanged in the modern nomenclature. The language of the peasantry proves to be much nearer to Aramaic or even to Hebrew than to the pure Arabic of Arabia proper. Not only does the pronunciation of various letters and words reproduce the Aramaic sounds, but many words in common use among the peasantry are of pure Aramaic origin, and are not used, or even in some cases not understood, by the townspeople who employ the more modern Arabic equivalents.

As a single instance the word *Jurn* may be noted. In Arabic it means a trough, but among the peasantry it signifies a threshing-floor, like the Hebrew *Goran*. The Arabic word used by the towns-folk and educated classes to signify a threshing-floor is *Nadir*, and it was not until after some time had elapsed that we discovered the meaning attached by the peasantry to the word *Jurn*. Many other instances might be quoted; but the general result seems to be that the peasant language in Palestine is almost unchanged since the times of Jewish domination. The preservation of the ancient nomenclature is thus easily explained, and the explanation is

confirmed by the fact, that the ancient names are as a rule irretrievably lost in districts inhabited by the Bedawin who immigrated at a late period from Arabia to the Syrian deserts.

Not only the language but the customs, dress, and religion of the peasantry are extremely archaic. The old worship of high places is still preserved among the villagers, sacrifices not in accordance with the doctrines of Islam are offered to local divinities, lamps are lighted, votive offerings suspended, and solemn dances and processions celebrated at the innumerable shrines which are found on every high mountain and under every large tree.

So close is the correspondence between the habits of the peasantry and the description of the habits of the indigenous population in Jewish times, that the Fellahin of Palestine may apparently be without improbability considered the descendants and representatives of the ancient Canaanite tribes.

The Zoology of Palestine has been made a special study by the well-known explorer Canon Tristram, and we had no hope of being able to add in any material degree to his discoveries. Fortunately, however, we were able to determine the existence of a species of deer not previously known to inhabit Palestine. By the natives it is called *Yahmûr*, a word identical with the Hebrew term rendered "fallow deer" in the Bible. A specimen of the *Yahmûr* was brought to us by the Arabs of Carmel, and the skin and bones were sent by Mr Tyrwhitt Drake to the Museum at Cambridge. The animal was there pronounced indistinguishable from our European roebuck, and we were thus able to ascertain the actual species of game which furnished the tables of Solomon with savoury venison.

The Geology of Palestine presents features of unique interest connected with the extraordinary depression of the Jordan valley. The country has never yet been thoroughly examined by a professional geologist, and although much has been done by M. L'Artet and by Canon Tristram, much still remains requiring skilled judgment to explain. The attention of the survey party in respect of this question was principally devoted to the general structure of the country and the distribution of the main divisions of the strata.

The hill country of Palestine is formed by a steep anticlinal, the strata belonging principally to the period of the Lower Chalk, and

including soft white marls and limestone overlying a hard crystalline dolomitic limestone of the Neocomian series.

Batches of nummulitic limestone belonging to the early Tertiary beds are found in Galilee, and on Ebal, Gerizim, and Olivet. On the western slopes of Lebanon and on the east side of the Jordan valley the Nubian sandstone belonging to the time of the Greensand is found, but this formation never appears west of Jordan.

The dip of the strata along the Jordan valley was very carefully noted during the prosecution of the survey. In every case a very sudden contortion of the strata was observable, the dip being eastwards or south east, and in places faults were found extending north and south. It was clear that the original depression had taken place after the Chalk period, and the basaltic outbreaks which surround the Sea of Galilee, and cover 500 square miles east of the lake, also belong apparently to the time of the first breakdown of the chasm in the early Tertiary period.

The appearance of the sandstone east of the valley bottom is considered by L'Artet a conclusive proof of the fact, that the whole depression is due to a fault running north and south for 150 miles, and giving a fall of 3500 feet from the springs of Jordan to the bottom of the Dead Sea.

The remains of an ancient beach were discovered by the survey party north of the plains of Jericho, and again south of the Sea of Galilee. Near the Dead Sea other beaches are visible at different levels, and it is clearly evident that the present valley was once occupied by a chain of great salt lakes, the surface of which was about on the same level with the Mediterranean, and which have at a comparatively recent geological period undergone a process of desiccation until they are now only represented by the smaller sheets of water known as the lakes of Merom and Tiberias and the Dead Sea, the extreme saltness of the latter being due to the gradual washing down of the chlorides from the basins now dry, a process which seems to promise the final consolidation of the Sea at a remote period into a bed of 500 square miles of salt.

The questions connected with the climate and physical geography of Palestine, its ancient fertility, its present desolation, and the possibility of its future restoration, are of still higher interest, and the survey seems to have thrown considerable light on these subjects also.

The impression produced on first entering Palestine is that of a barren country and of desolate ruins representing a former condition of prosperity. It must not, however, be supposed that the soil is wanting in fertility. The luxuriant growth of weeds and wild bushes sufficiently attests the richness of the land. In those districts where the soil consists of porous chalk, and where the water sinks down to the underlying impervious strata, the bareness of the country is very remarkable. In the higher mountains, where the dolomitic limestone is denuded, the western ridges are thickly clothed with copses of mastic and dwarf oak. In Sharon and Lower Galilee extensive woods of oak still exist; and although a great destruction of forest (which existed even as late as the twelfth century) has apparently occurred in some districts, there is a corresponding spread of the thickets in other parts of the country, where the sites of ancient vineyards and orchards are found overgrown with thick copses.

There does not appear to be any good foundation for the popular theory of a great diminution in the rainfall of the country. The average fall is now about twenty inches in the year, and all the famous springs noticed in the Bible are found still to yield a good supply. There are twelve considerable perennial streams in Palestine besides the Jordan, and many districts, such as the Hebron hills and the lowlands of Galilee, are plentifully supplied with springs. In the chalk districts no change in the supply can apparently have taken place within historic times, and the great number of cisterns and tanks, many of which are certainly of immense antiquity, gives evidence that it was necessary, even in the earliest historic period, to provide a large amount of storage for rain water in the districts not naturally supplied.

The change which has actually occurred in the climate and condition of the country seems to be less important than is sometimes supposed, and appears to be due principally to depopulation and to the decay of the ancient cultivation. The malaria of the lowlands is plainly traceable to the absence of proper drainage, and to the destruction of the ancient works of irrigation, many of which date back at least to the Christian era.

The swamps formed in the plain of Sharon are due to the filling in of ancient rock-cut channels, which once conducted the drainage

of the mountains to the sea, and the miasma in many cases arises from the stagnation of water in the torrent beds, which might with very little difficulty be drained into the sea.

Throughout Palestine, also, there are evidences of an extensive and careful cultivation now entirely abandoned, and of a population which has been estimated at not less than ten times the number of the present inhabitants. The sides of the hills are carefully terraced, though now often only growing thorns and thistles. Ancient wine presses and rude stone orchard towers are encountered in every direction, often on the sides of hills now entirely uncultivated. The ancient ruined towns and villages, so thickly strewn over the country, number more than ten times the present total of inhabited villages. The population, which does not exceed three millions for all Syria, is entirely inadequate for the cultivation of the country, and the villages are thus found standing in tracts of plough land or orchards surrounded on every side with waste ground or thick copse.

The riches of Palestine appear now as of old to be principally agricultural. The quality of the corn, wine, and oil is not inferior to that of even the south of Italy, and it can scarcely be doubted that, should any circumstances lead to the development of the natural wealth of the country, Syria might become an important source for the supply of the three products above mentioned.

The restoration of the country to a condition of prosperity depends, in short, not on any change in its climate, rainfall, or vegetation, but on the establishment of a just government, the liberation of the native peasantry from unjust taxation, violence, and oppression, and on the establishment of a condition of security which might induce the Jewish and other local capitalists to invest their money in the cultivation and irrigation of the land, in the development of its trade, and in public works which are at present entirely non-existent.

The examination of the ruined sites throughout the country formed one of the most important occupations of the survey officers. A note was made of every ruin which could be found, and a sketch or plan of every object of interest. The hopes which were naturally entertained of the discovery of remains belonging to the Jewish or Phœnician period were, however, doomed to disappointment, and the conclusion to which it seems necessary to submit is that the Jews

were not a people of great architectural genius. Large numbers of Byzantine and Crusading buildings were examined, and many structures previously attributed to an earlier period were clearly proved during the course of the survey to belong to the late times of Christian domination in the country.

With exception, indeed, of the rock-cut tombs and of the great walls of the Jerusalem Haram, not a single building was found which could be attributed to an origin earlier than the times of Herod the Great. The walls of Masada, the aqueducts of Cæsarea, the colonnade at Samaria, and the great building at Herodium, appear to be the work of this monarch; but the idea which finds expression in many books on Palestine, that all masonry with a sunk channel or draft round the stones is of Jewish or Phœnician origin, was plainly disproved by the observations taken during the course of the survey.

The ancient sepulchres formed a study of the greatest interest and importance, as serving to indicate roughly the date of ruins where they occur. The earliest Jewish tombs with *Kokim* or narrow graves running in from the walls of the chamber, were found to have been superseded about the time of Christ by another form of sepulchre, in which a rock-cut sarcophagus was excavated at the side of the chamber, while a cylindrical stone took the place of the older hinged or sliding door. The rock-cut tombs of the Christian period, fitted for the burial of two bodies—man and wife—are again quite distinct in form, consisting of graves sunk in the flat rock and covered with a great stone. Dated inscriptions were found on many of these tombs, with Christian emblems and leaden coffins.

Some light has also perhaps been thrown on the vexed question of the length of the Jewish measure called *Ameh* or cubit, by the careful measurements of the synagogues and of the Temple buildings at Jerusalem.

According to the Talmudists the cubit measured the length of 48 barley corns, which by measurement of barley corns in Palestine would represent 16 English inches very closely.

It was found that the pillars of the synagogues had in many cases a total height of 160 inches, or 10 cubits of 16 inches, the bases being 16 inches (1 cubit), the capitals 8 inches (half a cubit). In 1873 I was so fortunate as to discover a part of

the Haram Wall at Jerusalem not previously examined, where buttresses of ancient masonry are built at intervals. The interval from centre to centre was 160 inches or 10 cubits, and the dimentions of many of the great stones are in the same way multiples of a unit of 16 inches, which it is thus natural to conclude represents very closely the length of the medium Jewish cubit.

The limits of the present paper will not allow of any account of the exploration of Jerusalem. The survey of that city had been previously executed in 1864, by Col. Wilson, R.E., and the excavations of Captain Warren, R.E., had placed the topographical controversies on an entirely new footing. A certain amount of additional information was collected by the survey party, including 150 observations of the rock-levels in the city, which have an important bearing on some of the disputed questions, but the subject is too large to be further noticed in this paper.

As has been already stated, the identification of ancient sites, especially those connected with biblical history, formed one of the principal objects contemplated in undertaking the Palestine survey. The results in this field of research have been perhaps more satisfactory than could have been expected. About 170 new identifications have resulted from the survey, and it is satisfactory to be able to say that most of these have been well received by students of the subject, and pronounced valuable by good authorities. The number represents about two-fifths of the total of biblical sites now identified, the remainder being the results of the labours of the famous travellers Burckhardt, Robinson, Vandevelde, and others.

Among the places thus newly recovered, or concerning which fresh information has been collected by the survey party, may be mentioned the royal Canaanite cities of Hazor and Debir, Adullam, Lachish, and Megiddo, with the New Testament towns Emmaus and Salem. New information has also been collected as to Capernaum, which the survey officers are inclined to place at the site proposed by Dr Robinson, called *Minieh*, rather than at the traditional site of *Tell Hûm.* It would, however, be impossible to enter at length into the various interesting questions connected with these sites.

A single example of a survey identification may, however, be noticed in conclusion of this paper, as being perhaps the most interesting result of the survey of Palestine, namely, the recovery of

the supposed site of the Bethabara of the New Testament, a place where John the Baptist met our Lord, and where it is supposed that the baptism of Christ occurred.

Since the fourth century A.D., the traditional site of Bethabara has been always shown at the most southern ford of Jordan, due east of the present Jericho. This spot is annually visited by crowds of devout Russian or Syrian Christians, and the scene of their immersion in the river is extremely picturesque and has often been described by travellers. There is not, however, any conclusive evidence that the site so pointed out is genuine; the name Bethabara does not survive in the vicinity, nor is the place mentioned by any writers before the fourth century.

There is, moreover, a fatal difficulty in identifying the true and the traditional site; for it is clear from the Scripture narrative that Bethabara lay at not more than a day's journey from Cana of Galilee, which was situated north of Nazareth. We are thus limited to a distance of about twenty miles from Cana (which is probably the present village called *Kefr Kenna*), whereas the traditional site is no less than eighty miles from Cana, a distance representing three days' journey rather than one.

The name Bethabara is a compound of *Beth* "a house" and *Abara* "a passage" or ford. The village stood beyond Jordan, and apparently took its name from a neighbouring ford of the river.

The fords of Jordan were carefully examined during the course of the survey in the Jordan valley. No less than forty fords were found between the Sea of Galilee and the Dead Sea, of which only six are marked on former maps. Their names, which are mainly descriptive, were obtained, and care taken to ascertain as far as possible the exact positions. Some of the fords are only passable in summer or autumn, but others, to which the main roads lead, are practicable at all times except during heavy floods.

Among the fords, one and one only was found having the name 'Abârah, identical in form and in meaning with the name of the ford whence Bethabara took its title. This passage of the river, discovered by Sergeant Black in the ordinary course of the survey work, is situated about twelve miles south of the Sea of Galilee, at the place where one of the main roads from Lower Galilee crosses over into the district of Bathania or Bashan.

The name thus recovered is not a mere descriptive title in common use among the Arabs. No other ford is so called as far as could be ascertained by careful questioning, and the word 'Abârah does not occur again among the 10,000 names collected within the limits of the survey.

The distance from the ford 'Abârah to the probable site of Cana of Galilee is about twenty-two English miles, the road being the shortest and easiest leading from that town to any part of Jordan. There is thus a possibility of journeying in a single day between the two places, which, as before mentioned, agrees with the account given of Bethabara in the gospel narrative, but which is not in accordance with the position of the traditional site near Jericho.

This discovery is a fair sample of the biblical results due to the survey. The discoveries of similar character connected with the Byzantine and Crusading history of the country are not less numerous or interesting; but I hope that what has now been said may serve to show the aims of the work, and that—when the difficulties connected with its execution are borne in mind—the results may be considered adequate for the time and money expended on the survey.

In conclusion, I would venture to say that the success of the work should have a peculiar interest for Scotsmen, for although the leaders could not claim Scotch descent, it is to the zeal and faithfulness of the two sergeants, Black and Armstrong, that the thoroughness and accuracy of the survey are in great measure due, and both these non-commissioned officers, as well as others of the staff, were natives of the north side of the border.

2. On Minding's System of Forces. By Professor Chrystal.

(*Abstract.*)

Minding has proved a remarkable theorem concerning a variable system of forces defined as follows, the points of application of the different forces, and their magnitudes are given, while the directions are such that a pencil of rays through any given point parallel to them moves as a rigid body.

Besides Minding's original investigation, several others have been

given since. The last of these, due to Professor Tait, rests on purely quaternion methods, and is so elegant and concise that I was led to reinvestigate the whole subject by ordinary methods in the hope that the analysis might have some points of interest. Two methods of arriving at Minding's result are given, and a variety of other conclusions are arrived at by means of the second method sufficient to indicate the course of a full investigation of the complex formed by the central axes, and of the congruency formed by the single resultants of Minding's system.

First Method.

The components of force and couple are found in terms of the Rodrigues co-ordinates $\lambda\mu\nu$, which determines the position of the rigid pencil representing the direction of the forces.

The equations to the single resultant are then found in terms of two constants g and h, and the parameters $\lambda\mu\nu$.

Equations are then deduced for the values of $\lambda\mu\nu$ corresponding to a ray passing through a point xyz. Eliminating μ and ν a biquadratic is found for λ. The system of resultant rays therefore forms a congruency of the fourth order.

This biquadratic becomes wholly indeterminate for points on the real focal conics of the ellipsoid

$$\frac{x^2}{g^2+h^2} + \frac{y^2}{h^2} + \frac{z^2}{g^2} = 1 \quad . \quad . \quad . \qquad \text{(A)}$$

Some farther discussion leads to the conclusion that the resultant rays of Minding's system is identical with the congruency of rays that intersect the two focal conics of (A).

Second Method.

If $\xi\eta\zeta$ be the co-ordinates of the feet of the perpendicular from the origin on any ray whose direction is (λ, μ, ν), and ρ the length of that perpendicular, it is shown that

$$\rho^2 = g^2\mu_1{}^2 + h^2\nu_1{}^2 \quad . \quad . \quad . \qquad \text{(B)}$$

$$\rho^4 + g^2\eta^2 + h^2\zeta^2 = g^4\mu_1{}^2 \div h^4\nu_1{}^2 \quad . \quad . \qquad \text{(C)}$$

(B) is true for central axes generally, and determines a complex of the second order which they form. Both (B) and (C) are true for

the rays of single resultant, and are the twofold relation which determine a congruency with which they are identical.

A discussion is given of the complex determined by the relation

$$\rho^2 = f^2\lambda_1{}^2 + g^2\mu_1{}^2 + h^2\nu_1{}^2 \quad . \quad . \quad \text{(D)}$$

of which (B) is a particular case.

The equations to Plücker's complex cone and equatorial and meridian surfaces are given, and various loci connected with the complex are discussed.

A method of exploring the complex by means of central radii is then given.

It is found that the stretch on any radius that is intersected by rays of the complex perpendicular to that radius is in general finite.

An equation for the distances of the ends of this stretch from the origin is found, and expressions for the direction cosines given for the extreme rays which are at right angles to one another.

Various results concerning the lengths of perpendiculars are given; among them that the sum of the squares of the perpendiculars on three rays mutually at right angles to each other is constant.

The solid locus of the feet of the perpendiculars on the central axis generally is found to be the space between the sheets of the surface

$$\frac{x^2}{r^2 - f^2} + \frac{y^2}{r^2 - g^2} + \frac{z^2}{r^2 - h^2} = 0 \quad . \quad . \quad \text{(E)}$$

which is the reciprocal of the wave surface.

Lastly, the congruency of rays determined by (D), and the additional relation

$$\rho^4 + f^2\xi^2 + g^2\eta^2 + h^2\zeta^2 = f^4\lambda_1{}^2 + g^4\mu_1{}^2 + h^4\nu_1{}^2 \quad \text{(F)}$$

is discussed, and shown to be of the fourth order. Minding's theorem is shown to hold when $f = 0$. (It is not true when $f \mp 0$). The equation to the surface locus of the feet of the perpendiculars on the rays of resultant is found, and so far as mere inspection goes is of the twelfth degree. In conclusion, the equations of various other loci connected with the congruency are given, or indicated to show the power of the methods employed.

Many of the above results were previously obtained quaternionically by Professor Tait. The interest of the present communication is less in the results obtained than in the methods employed to treat a particular problem in Plücker's "Line Geometry." In the development of the results Plücker's "Neue Geometrie" has been followed as far as possible. Any interested reader may be referred to that work for farther information on this and like matters.

3. Mathematical Notes. By Professor Tait.

(*a*.) On a Problem in Arrangements.

While making some algebraic problems last summer for an examination, I devised the following question :—

"A schoolmaster went mad, and amused himself by arranging the boys. He turned the dux boy down one place, the new dux two places, the next three, and so on till every boy's place had been altered at least once. Then he began again, and so on; till, after 306 turnings down, all the boys got back to their original places. This disgusted him, and he kicked one boy out. Then he was amazed to find that he had to operate 1120 times before all got back to their original places. How many boys were in the class?"

It is clear that one of the factors of the number of turnings down is $(n-1)$, where n is the number of boys in the class. The factors of 306 are 18 and 17, and those of 1120 are 7, 10, and 16. If we try 17 as the original value of $n-1$, 16 will be the value for one boy less: from which it appears by a tentative process that the class consisted of 18 boys. But it is interesting to examine the nature of the question more closely. It is intimately connected with one of the problems suggested in my paper on "Knots" (Trans. R.S.E., 1876–77, § 5). If we know the arrangement of the boys—after one of them has for the first time been turned to the foot of the class, the processes given in that paper lead easily to the complete solution.

Now it is easy to see that the particular arrangement just mentioned can be found diagrammatically as follows :—

Write down the numbers

$$1 \quad 2 \quad 1.$$

Put the double of the middle number to the right of it, and the next lower number to the left. Thus

$$1 \quad 3 \quad 2 \quad 4 \quad 1.$$

Operate in the same way on the numbers *last introduced*, and we have

$$1 \quad 5 \quad 3 \quad 6 \quad 2 \quad 7 \quad 4 \quad 8 \quad 1.$$

Continue in this way, and arrange these groups in successive order, leaving out the final 1 from each. We thus have the series

1, 2, 1, 3, 2, 4, 1, 5, 3, 6, 2, 7, 4, 8, 1, 9, 5, 10, 3, 11, 6, 12, 2, 13, 7, &c.

Strike off the first $n-1$ of these numbers (n being the number of boys), and the next n represent the arrangement of the class after all have been displaced: the numbers designating the several boys by their original places. Hence we have the key for translating the series into the successive derangements.

Another curious mode of getting this series is to begin with 1, then prefix 1, and insert 2, as below:—

$$1 \quad 2 \quad 1.$$

Again prefix 1, and insert 2, 3, 4, then

$$1 \quad 2 \quad 1 \quad 3 \quad 2 \quad 4 \quad 1,$$

and so on indefinitely.

It is worthy of remark that this series gives the integral of the equation

$$u_{2x+1} = u_x;$$

with the conditions

$$u_{2x} = x + 1,$$

$$u_1 = 1;$$

i.e., the solution of the following question:—

"Arrange an infinite row of numbers, those in the even places being 2, 3, 4, &c., so that if the first $(n-1)$ be struck off (n being any integer) the next n may consist of all the natural numbers from 1 to n inclusive."

Another result which these numbers present is the following:—

Every positive integer can be expressed, in one way only, by the sum of a finite number of terms of one of the infinite set of series

$$\begin{array}{c} 1 + 2 + 4 + 8 + 16 + \\ 2 + 3 + 6 + 12 + 24 + \\ 4 + 5 + 10 + 20 + 40 + \\ 6 + 7 + 14 + 28 + 56 + \\ 8 + 9 + 18 + 36 + 72 + \\ \text{\&c., \&c.,} \end{array}$$

the partial sums for each being the several places occupied in the above series by each particular integer. This, however, is obvious when we consider that the sum of $(n+1)$ terms of any one of these series is of the form

$$(2r + 1)2^n - 1,$$

and that this expression can be made to equal any given positive integer by one definite pair (and one only) of values of r and n.

Thus we see that we may write

$$u_x = \tfrac{1}{2}(1 + \underline{x + 1}),$$

where the bar under $x+1$ means that it is to be divided by the highest power of 2 that it contains.

The numbers of operations, for classes of different numbers of boys from 2 to 25 inclusive, are in order as follows :—

2, 4, 9, 20, 30, 36, 28, 72, 36, 280, 110, 108, 182, 168, 75, 1120, 306, 432, 190, 140, 4410, 2772, 2530, 1440.

The calculation of the numerical value of any particular term is easy, but I have not attempted to express the general law of this very curious series. It seems, however, to be well worthy of attention, especially from the point of view of the expressions for numbers in the binary scale.

(*b.*) On a Graphical Solution of the Equation $V\rho\phi\rho = 0$.

This equation has been exhaustively treated in our Transactions by M. G. Plarr. The present note is a mere sketch of a graphical

solution. Let ϕ be divided into parts, one self-conjugate, the other not, then

$$\phi = \varpi + \mathrm{V}.\epsilon\,,$$

and the given equation may be written

$$\varpi\rho + \mathrm{V}\epsilon\rho = x\rho\,.$$

Hence $$\mathrm{S}.\rho\left\{(\varpi - x)\alpha + \mathrm{V}\alpha\epsilon\right\} = 0$$

whatever be α. Let α, β, γ be the principal unit-vectors of the pure strain ϖ, and a, b, c (in descending order of magnitude) the associated scalars. Then the equation for x is, at once,

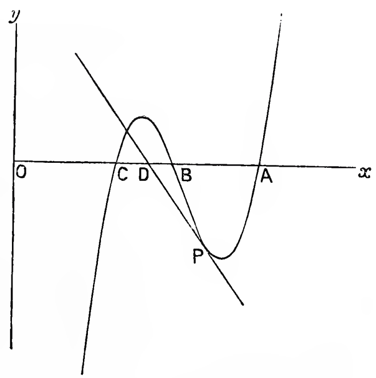
$$\mathrm{S}.\Big((a - x)\alpha + \mathrm{V}\alpha\epsilon\Big)\Big((b - x)\beta + \mathrm{V}\beta\epsilon\Big)\Big((c - x)\gamma + \mathrm{V}\gamma\epsilon\Big) = 0.$$

This may be written as

$$(x - a)(x - b)(x - c) - \epsilon^2(x + \mathrm{S.U}\epsilon\,\varpi\mathrm{U}\epsilon) = 0\,.$$

Thus the problem is reduced to finding the limiting value of $\mathrm{T}\epsilon$, for any given value of $\mathrm{U}\epsilon$, so that the above equation may have all its roots real. This leads by the ordinary methods to a cubic in $\mathrm{T}\epsilon^2$, but the expression is rather complicated.

For variety let us adopt a graphic method. It is obvious that the extreme values of $-\mathrm{S.U}\epsilon\,\varpi\mathrm{U}\epsilon$ are a and c.



Let the curve represent the equation

$$y = (x - a)(x - b)(x - c),$$

and let OD represent any assumed value of $-\mathrm{S.U}\epsilon\,\varpi\mathrm{U}\epsilon$. D must lie on the finite line AC. From D draw, as in the figure, a tangent

DP to the curve; and suppose a simple shear to be applied to the figure, parallel to the axis of y, so as to make this tangent coincide with the axis of x. The equation of the curve after the shear will obviously be

$$y = (x - a)(y - b)(z - c) + \tan \widehat{PDA}\,(x - OD)$$

and it will touch the axis of x. Comparing this with the equation above, we see that we have for the maximum value required

$$T\epsilon^2 = \tan \widehat{PDA}.$$

The absolute maximum of $T\epsilon$ is obviously when the point of contact is the point of inflexion of the curve (whose abscissa is $\frac{1}{3}(a+b+c)$), and the least values when D coincides with C or with A. These values are easily seen to be, in order,

$$\frac{a-c}{2}\sqrt{1+\tfrac{1}{3}\left(\frac{a-2b+c}{a-c}\right)^2},\ \frac{a-b}{2},\ \text{and}\ \frac{b-c}{2}.$$

BUSINESS.

Professor TURNER proposed the following motion, of which he gave notice at the General Statutory Meeting:—

"That the Honorary Vice-Presidents be in future members of the Council of the Royal Society, and that the Laws of the Society be modified to the extent necessary to carry this into effect."

This was seconded by Professor TAIT, and agreed to.

It was moved by Mr FERGUSON of Kinmundy, seconded by Professor DUNS, and agreed to—

"That it be remitted to the Council to bring up to the next Meeting the verbal alteration of Law XVII. required to carry out the above resolution."

Professor TURNER proposed the second resolution, of which he gave notice at the General Statutory Meeting, viz.:—

"That the Chairman of the Meetings of the Council should have a casting as well as a deliberative vote."

This was seconded by Professor TAIT, and agreed to.

Thomas Armstrong Elliot, M.A. Cantab., was balloted for, and declared duly elected a Fellow of the Society.

Monday, 19th January 1880.

Professor H. C. FLEEMING JENKIN, Vice-President, in the Chair.

The following Communications were read and Business transacted:—

1. Part of the Material employed by Principal Forbes in Tamping the Bore for his Earth Thermometers at the Edinburgh Observatory was exhibited in its altered state.

2. A New Method of investigating Relations between Functions of the Roots of an Equation, and its Coefficients. By J. D. H. Dickson, M.A.

3. Remarks on Mr Crookes's Recent Experiments. By Professor George Forbes.

The author explained that he was glad to be able to bring before the Society some apparatus illustrative of Dr Crookes's splendid researches on what he calls *radiant matter*. This he was able to do by the skill of Mr Gimingham, who had prepared some splendid vacuum tubes for him lately. He valued those researches very highly, because they brought most conclusive evidence to bear upon the truth of the kinetic theory of gases; because they gave us a clear insight into the action of the electric current and discharge; because they promise to help us in forming a notion of what electricity really is; and because we may also hope from these researches to get some knowledge of the nature of molecules.

It is now generally conceded that a gas consists of detached molecules, moving about with great rapidity, and rebounding from each other and from the sides of the containing vessel, with perfect elasticity; Clausius and Maxwell have shown that at any given pressure the molecules on an average can travel over only a certain

distance without experiencing a collision. In the case of hydrogen, at atmospheric pressure, this distance, called the *mean free path* of a molecule, is the $\frac{1}{250000}$th part of an inch. If the hydrogen in a tube at atmospheric pressure be gradually removed by an air-pump until only a small fraction of that quantity of gas remains in the tube, then each molecule can on an average traverse a distance much greater without having a collision with another molecule. During the time of passing over this space their vibrations are more or less given off to the other. Thus they soon cease to be luminous, even if incandescent when they last collided with a molecule. These facts explain the action of the radiometer.* They also explain the fact observed in *very* high vacua, that there is no luminosity round the negative pole† of a vacuum tube (highly exhausted) when two poles inside it are connected with the poles of an induction coil.

[The first experiment was here shown, when it was seen that in a very perfect vacuum no light was seen in the tube except a phosphorescence of the glass tube, due to the impulse of the molecules of gas upon it. When water-vapour was admitted by heating a subsidiary bulb containing caustic potash, the ordinary phenomena of vacuum tubes were gradually developed.]

The impact of these molecules on German glass or other phosphorescent substance produces intense phosphorescent light. [This was shown by placing in these vacuum tubes phosphorescent sulphides and rubies, and by directing the discharge upon German glass.]

When the electric density of a considerable portion of the negative electrode is uniform, we should be led to expect that the molecules of gas which strike it should be driven off perpendicular to the surface, that being the direction in which the repulsive action between the electrified electrode and the similarly electrified molecules should take place. This is exactly what happens. If the negative electrode be a circular disc, the molecules are driven off to the opposite side of the tube or bulb. This direction depends in no way upon the position of the positive pole. In this case the author remarked on the existence of a black mark in the centre of

* See Tait and Dewar, *Nature*, 1875. But also see Clerk Maxwell, R.S., 1879.

† The molecules of gas certainly fly off from the negative pole in obedience to the above laws. At present we know little about what happens at the positive pole.

the luminous phosphorescent patch produced on the glass by the molecular bombardment. When this is seen it is due to a stream of blue light from the centre of the negative disc. This stream is not deflected by a magnet as is the stream producing phosphorescence. Since the molecules are driven off perpendicular to the surface of the negative electrode, any object placed in their path shields the phosphorescent glass from the molecular bombardment, and thus a shadow of the object is thrown on the glass. The author drew attention to the fact that there was no penumbra to this shadow. Considerable heat is developed on the glass or any other object exposed to this molecular bombardment. If the negative electrode be a concave part of a sphere, the molecules are concentrated at the centre of the sphere, and produce there sufficient heat to melt platinum.

We know from the experiments of Rowland that a body charged with electricity, when in motion, acts like an electric current. So here these charged molecules in motion are deflected by a magnet as a current would be. But they do not yield to the electromagnetic force so much as a perfectly flexible linear conductor would do.

The author showed that the stream of molecules, when falling on glass, could be reflected on to a piece of mica covered with a phosphorescent powder; but he has not yet been able to determine whether they are reflected according to the laws of the reflection of light. He also drew attention to certain small black spots which he has occasionally noticed on the phosphorescing glass which move over it like bubbles, disappear, and are succeeded by others.

He suggested that efforts should be made to determine what happens to molecules at the positive pole—also to discover by what means the negatively electrified particles, after having been projected along the length of a tube, find their way back again, if not by regular reflection.

4. Additional Note on Minding's Theorem. By Professor Tait.

BUSINESS.

Professor Turner reported that in accordance with the remit made by the Society at last Ordinary Meeting, the Council recommended the following changes in the Laws of the Society:—

"That Law XVII be cancelled, and the following Law be established in its place:—

LAW XVII.

"That there shall be formed a Council consisting, first, of such gentlemen as may have filled the office of President, and, secondly of the following to be annually elected, viz., a President, six Vice-Presidents (two at least of whom shall be resident), twelve Ordinary Fellows as Councillors, a General Secretary, two Secretaries to the Ordinary Meetings, a Treasurer, and a Curator of the Museum and Library."

That the following addition be made to Law XV.:—

"The Council shall have power to regulate the private business of the Society. At any Meeting of the Council the Chairman shall have a casting as well as a deliberative vote."

Professor Turner moved, and Mr Sang seconded the adoption of these recommendations, which were agreed to.

The following Minute, prepared by the Committee appointed for the purpose by the Society at the General Statutory Meeting, was read and approved December 1, 1879. The Secretary was directed to send an extract to Dr Balfour:—

"The Members of the Royal Society of Edinburgh most cordially unite with the Council in acknowledging the long and valued services of Dr Balfour as General Secretary of the Society; they desire, as a body, to express how deeply they regret the loss of these services; and whilst sympathising with him in the circumstances which have led to the resignation of the office he has so long held in the Society, they would cherish the hope that he may enjoy in his retirement that serene repose which the consciousness of a well-spent life is fitted to insure."

The following letter in reply was now read:—

"Dear Professor Tait,—I feel highly honoured by the kind recognition, by the Fellows of the Royal Society, of my long

continued services as their Secretary, and I return my warmest thanks for the communication which you have transmitted to me.

"Be so good as convey my kind acknowledgment to the President and Fellows of the Royal Society.—I am, your obedient servant,

"J. H. Balfour."

Monday, 2d February 1880.

Professor DOUGLAS MACLAGAN, Vice-President, in the Chair.

The following Communications were read:—

1. On the Distribution of Temperature under the Ice in Frozen Lakes. By John Aitken.

In January and February 1879 Mr J. Y. Buchanan communicated to this society two papers, on the distribution of temperature under the ice in Linlithgow Loch. In these papers he gives a most interesting and valuable series of temperature observations made by him, of the water, at different points, and at different depths, in the loch while it was covered with ice. He also gives the temperatures as taken by him under similar circumstances in Loch Lomond. These observations by Mr Buchanan disclose a somewhat unexpected thermal condition of the water.

As water attains its maximum density at a temperature of 39·2° Fahr., it has generally been supposed that water in lakes ought never to fall below this temperature, except near the surface. Because when the water is cooled below 39·2° it will float over the hotter and denser water, and tend to keep the surface, while the latter water will tend to keep the bottom, and radiation and conduction will only enable a lower temperature than 39·2° to penetrate below the surface, very slowly, and to a very small depth. These theoretical expectations are, however, entirely upset by Mr Buchanan's temperature observations. He found that the greater part of the

water under the ice in Linlithgow Loch had only a temperature of about 37°, while in Loch Lomond the mean temperature was as low as 34°.

The object of the present communication is to offer an explanation of this unexpected condition of the water—to show how theory has failed correctly to predict the thermal conditions of a frozen lake. Mr Buchanan in the papers already referred to, and also in "Nature" of 6th March 1879, gives an explanation of how water in lakes becomes cooled below its temperature of maximum density. As this theory is at variance with many well-known results of experiment, and does not seem satisfactorily to explain the facts, it will be necessary first to show wherein this explanation fails. Mr Buchanan's theory is simply this:—The water in the lake gets gradually cooled to the temperature of its maximum density, namely 39·2°. Some time after all the water has acquired nearly this temperature, freezing begins at the edge first, while there is open water in the middle of the lake. The effect of this freezing would be expressed graphically by the dipping of the isothermal of 39·2° at the edge. This alteration of the temperature will be accompanied by an alteration in density; and if we consider a vertical section in the middle of the lake and another section at the edge, we will find the mean density at the middle greater than at the edge, the result of which is convection currents, flowing on the surface from the ice, and under currents from the middle towards the sides. This appears to be a perfectly correct statement of the condition of the water under the circumstances, but it does not seem capable of giving an explanation of the temperature of the water all through the lake being below 39·2°. The colder and lighter water near the edge, where freezing first begins, will certainly tend to spread itself towards the middle of the lake—but it will tend to keep the surface, and will not sink and produce a vertical circulation. And unless this cold surface water sinks how is the lower temperature to be carried downwards? We have here a precisely corresponding condition of matters to what exists in the lake on the return of summer, when the water is being heated. At first the temperature of the lake is below 39·2°, and the warmer water brought in by the rivers and that heated by the sun, sinks to the bottom and raises the mean temperature of the lake to the temperature of its maximum density. After it

has attained a temperature of 39·2°, the warmer inflowing water no longer sinks, but spreads itself over the colder water and keeps its place on the surface, and does not give rise to a vertical circulation, and carry its higher temperature down into the depths of the lake.

As theory might not have taken into consideration all the conditions which exist in a lake while freezing, an experiment was made, in which the conditions of a freezing lake were imitated as closely as possible, to see if Mr Buchanan's theory was correct, and to confirm or correct our previous knowledge. A trough 46 cm. long by 15 cm. broad and 12 cm. deep was filled with water. In the trough were fixed three thermometers, one was placed 1·5 cm. from the surface, one in the middle, and one 1·5 cm. from the bottom. The trough was made with glass sides, so that by dropping some coloured solution into the water, the direction, position, and velocity of the currents could be noted from time to time. Over the trough was placed a refrigerator, one end of which dipped into the trough and touched the water for a length of about 12 cm. This trough represented in miniature a section of a lake in which might be observed all the changes in currents and temperatures which take place due to the heating, cooling, and freezing of the water. Observations of temperature and currents were made and recorded from time to time during twenty-two hours. All these observations were in keeping with the theoretical knowledge we at present possess, and did not give support to the theory that the cooling of the water below 39·2° could give rise to convection currents which could carry their lower temperature into the depths of the lake.

The apparatus being arranged as described, the trough was filled with water at a temperature of 52·5° Fahr. A freezing mixture was put in the refrigerator; when this was done, currents at once began to flow. The surface current flowing *towards the cold*, that is towards the end at which the refrigerator touched the water, and the return current flowing near the bottom. These currents kept moving for two hours, during which time *all* the thermometers fell at very nearly the same rate, and at the end of the two hours they all indicated a temperature of about 41·5° The circulation during this time gradually got slower and slower, on account of the difference of density of the water on which the existence of these currents depended gradually diminishing.

A little later after the water had acquired nearly the temperature of its maximum density a change took place. In addition to the two currents described, another current began to set in, flowing on the surface and over the other two currents already described. Its direction being *from the cold*, that is in the opposite direction to the previous surface current. Its return current coming back with the previous surface current. This new surface current was produced by the water being cooled below its maximum density, and rising, instead of sinking as before. The effect of this new surface current was also indicated by the upper thermometer, which began to fall much faster than the other two. These two sets of currents kept flowing for some hours, gradually getting slower and slower, particularly the lower one, which seemed to stop after about four hours from the beginning of the experiment, by which time the water at the bottom had arrived at a temperature of about 40°. After eight hours almost all circulation had ceased even by the surface.

While the temperature of the water was above 39·2°, the heat was distributed by convection currents, causing all the water to circulate, and the temperature fell at about the *same rate at all depths*, and further it fell from 52·5° to 41·5°, or 11° in two hours. After the maximum density had been attained, and the cold current, instead of sinking to the bottom, flowed over the surface, the upper thermometer fell 7° in two hours, while the bottom thermometer during eight hours only fell 2°, being only 39·5 at the end of that time.

This experiment proves that the downward convection of heat ceased about the temperature of maximum density, as the lower thermometers ceased at about that temperature to fall at the same rate as the upper one. It is true the lower thermometer did fall below 39·2°, but it did so only very slowly, part of the fall being probably due to convection currents caused by the heat of the sides of the trough taking up the dense water, its place being supplied by the overlying colder water. It must also be remembered that the lower thermometer was only 10·5 cm. from the surface, so that the bottom water would lose part of its heat by radiation and conduction.

On removing the refrigerator and applying heat a reverse series of phenomena was observed. The hot water sunk and gave rise to a circulation, the surface current being *towards* the heat. The

return current was also near the surface, as the water was not heated to its maximum density it was unable to sink into the colder water at the bottom, and so flowed back over it and under the surface current. While this was taking place the upper thermometer was rising rapidly, while the lower ones did not show any signs of rising for some time. As the temperature of the surface water increased the return current got deeper and deeper, till it flowed at the bottom. The bottom thermometer then began to rise. This current continued to flow till the bottom water had acquired the temperature of maximum density, being some degrees below it previously. After all the water had acquired nearly the temperature of maximum density, a change took place. A new current began to flow on the surface *from* the heat, and over the previous surface current. The lower currents gradually became slower and slower, and at last stopped. After a time the surface current also ceased and all was still, the hot water resting quietly on the cold.

It is quite possible that the dipping of the isothermal of 39·2° referred to by Mr Buchanan, might give rise to a circulation of the water, but the conditions necessary for it to do so are not likely to happen, owing to the bad conducting power of water. The isothermal of 39·2° would require to dip to a great amount, to a number of feet *lower at the sides than in the middle of the lake*, in order to give rise to a sufficient "head" to put the water in motion. And even when a circulation is so produced, *the water flowing towards the cold region will not be drawn upwards from the bottom, but will flow in horizontally*, and will be water of a lower temperature than 39·2°; that is, the lower returning current will descend but little below the lowest point of the 39·2° isothermal, and all water at a lower level than this isothermal will be at rest, and retain its temperature of maximum density.

This point was illustrated in the experiment described. After ice had formed under the end of the refrigerator, and after the bottom water had acquired a temperature of about its maximum density, and was quite still, a current was noticed flowing slowly from the ice at the surface, and a return current immediately underneath; but more than a half of the water in the very shallow trough was unaffected by it, showing that the dense water does not rise from the bottom to keep up such a current, but that its supply is drawn from the

colder and lighter overlying water. After some hours even this surface current became so weak as not to be noticeable.

Convection currents being thus shown to be incapable of lowering the mean temperature of the water in lakes below 39·2°, I shall now call attention to a cause which, though not existing and not recognised in our laboratories, is yet in constant action in nature, and is quite sufficient to account for the thermal disturbance under consideration. The great vertical distributor of temperature in lakes is in all probability the winds which are constantly blowing over them. Previous to the freezing of the surface the winds were in constant action, producing currents in the water, which gave rise to the peculiar distribution of temperature we find after the lake is covered with ice. I have shown in a previous paper* that the great wind-driven currents of the ocean have but little influence on the vertical distribution of temperature of the oceanic waters. The circumstances are, however, quite different in lakes. In the ocean the wind only affects one part of the surface of the water, and gives rise to a circulation the greater part of which is *horizontal*, the return current coming back at a part of the ocean unaffected by the wind. But in a lake the wind blows in the same direction all over the lake, and the return current cannot come back on the surface, it is therefore compelled to sink and return *underneath* the surface wind-driven current. The cold surface water is thus compelled to sink and carry its low temperature into the depths of the lake. When we consider that water of a few degrees above and of a few degrees below 39·2° will have almost the same density, we will see that difference of density will oppose but little resistance to these wind-driven currents. And under almost all circumstances the "head" produced by the wind will be greater than that due to difference of density.

The temperatures as given by Mr Buchanan, so far as they go, are entirely in support of this wind theory. For instance, Linlithgow Loch is much smaller and better sheltered from the wind than Loch Lomond, and we find the mean temperature of Linlithgow is higher than Lomond, which we would quite expect, as it will have a less wind-driven circulation. Again, as Linlithgow Loch is smaller than Loch Lomond, the wind-driven surface currents will not be so deep, nor the return currents so near the bottom. We would therefore

* Proceedings of the Royal Society, Edinburgh, 1876–77.

expect the water at the bottom of Linlithgow Loch to be warmer than the water at the bottom of Loch Lomond, which we find to be the case.

[*Added February* 24, 1880.—It is scarcely necessary to say that the explanation here given of the descent of the cold water into the depths of lakes in winter, applies to the converse process which takes place on the approach of summer. If the distribution of temperature in lakes depended on currents due to difference of temperature alone, the water in the depths of lakes would never rise above 39·2°, as the sun's rays are robbed of their heating effect by a small depth of water, —and it is principally by the wind-driven currents that the hotter and lighter surface water is caused to sink into the depths of the lake].

2. Remarks on the Aborigines of the Andaman Islands. By Surgeon E. S. Brander, M.B., C.M., H.M. Bengal Medical Staff, Second Medical Officer, Port Blair and Nicobars. (Plate XV.)

I propose making a few brief observations on the relations in which we stand to the aborigines of the Andaman Islands, their place in the economy of this convict settlement, and their more noticeable habits and customs. In the consideration of the latter, I shall only state such information as I have obtained myself from conversation with, and personal observation on the manners of the natives. Detailed monographs on their ethnology, &c., have been written already by several observers, and are to be found in the "Transactions of the Anthropological Society," &c.

When the British Government first acquired possession of these islands, the aborigines, due perhaps to previous bad treatment at the hands of Europeans, presented a decidedly hostile aspect. Any attempt to land from ships was met by determined resistance on their part, and for some years this condition of things remained. It was shortly after the Mutiny that Government first decided to use these islands as a convict settlement, deeming it a safe measure to get the imprisoned mutineers safely out of the country altogether. By the time the convict settlement was first established, the Andamanese had ceased any hostile efforts, but had entirely fled into the jungle, where they refused to accede to any friendly overtures. By

the judicious leaving of presents for them, and by the habitual kindness of reception shown to any of their number venturing into the settlement, this timidity was gradually overcome. At that time a zealous and devoted servant of the Government, Mr J. Homfray, voluntarily went into the jungle and lived amongst these savages as one of themselves. Ignoring the discomforts of the position and thoroughly ingratiating himself with them, he remained in their midst until he acquired a fluent knowledge of their imperfect but difficult dialect. By this means he established in them a confidence and friendly feeling towards the "strange white people." The policy of this course was soon apparent. These islands were destined to be a great convict settlement, and the co-operation and friendliness of the aborigines with the British Government would at once strengthen the hands of the latter in the management of the place, by rendering escapes practicably impossible.

At the present time all the tribes in the neighbourhood of Port Blair are on most friendly relationship with the Executive here. One of the settlement officers has it, as his special duty, to look after them. On one of the islands here, called Viper Island, there is a "Home" for the reception of such healthy members of the jungle tribes as like to come in. Here they are well housed and fed. In exchange they make bows, spears, and various rude ornaments, spear and store up turtle, &c., and by the sale of these augment the Government grant allowed for their support. There is also a school for Andamanese children on Ross Island (the chief station here), where they are taught to read, write, and speak English and Hindostani, together with the elements of arithmetic, &c., and the girls in addition, needlework. Besides this there are the Andamanese Hospital and the Convalescent House on Viper Island, which were under my direction when I was in the capacity of Additional Medical Officer. While in the latter function, I resided myself on Viper Island, and so had considerable opportunities of observing the Andamanese both in hospital and in their normal condition. Since my promotion to the duties of Second Medical Officer, their medical charge has unfortunately left my hands.

Finally, the Andamanese at present most efficiently fulfil the function of always having 20 to 50 armed men ready on the shortest notice, to scour the jungle and recover, dead or alive, any escaped

convicts. For each convict so recovered Government pays them five rupees. In the great majority of cases the runaways are easily recaptured without any resistance, being generally exhausted from exposure and want of food. Cases, however, have occurred where the escaped convicts have shown fight on being overtaken, and when the Andamanese have had to use their bows and arrows (of which I shall speak after) with rapidly fatal effect.

Such then, briefly, is the present relation in which these people stand to us, and their general function in the community. I may add that yearly they are becoming more tractable. Tribes from remoter parts of these islands are voluntarily sending in messengers; these are always well treated, and take away with them useful presents and a good opinion of their entertainers. In time, then, we may hope to stand in a friendly relation to all these tribes, though some still remain who refuse any attempts at conciliation.

I will not make any detailed remarks on the structural or other affinities of these people. These have been treated of in the detailed monographs to which I before referred; I will therefore at once proceed to my personal experiences. One physiognomical fact has appeared to me very noticeable, however, and that is, the remarkable diversity of the facial type. Some faces seem to resemble the Negraic, some the Malayan, and some even the Aryan in character. A reference to the accompanying photo-lithographic illustration (fig. 1) will help to render this clearer. Thus the "characteristic," *i.e.*, the most frequent, type of Andaman face would seem to be of a modified Negraic variety. I may add, however, that I have noticed less dissimilarity to exist between the women's faces than the men's. As an example of the variety of facial character observable, I would point out the man's face seen in profile in fig. 1, with its comparatively straight nose and compressed lips, as distinguished from the man's face (seen full face) next to him with the flat nose and thick protuberant lips. When a number of these people are seen together this peculiarity becomes very noticeable.

In size these people are remarkably small, and I append some tables of measurements I have made. From these it will appear that the average man does not exceed 4 feet 10 inches in height, with a weight of about 101 lbs.; and the average woman, 4 feet 6 inches, with a weight of about 98 lbs. From the figures in the table, I

find that the greatest height registered for a man was 5 feet 1½ inch, lowest height, 4 feet 7½ inches, giving a mean of 4 feet 10·43 inches for men's height.

The heaviest man measured was 117 lbs., the lightest 92, giving a mean of 101·4 lbs. for men's weight.

Men.				Women.			
No.	Weight in lbs.	Height.		No.	Weight in lbs.	Height.	
		Feet.	Inches.			Feet.	Inches.
1	117	5	...	1	94	4	5
2	112	4	11½	2	103	4	7
3	106	4	11½	3	96	4	7
4	106	4	11½	4	91	4	6
5	95	4	9½	5	93	4	5
6	100	4	10	6	102	4	4
7	92	4	8	7	89	4	5
8	96	4	10	8	85	4	4
9	97	4	8½	9	92	4	7
10	98	4	11	10	96	4	3½
11	94	4	8	11	126	4	9
12	90	4	7½	12	103	4	6
13	109	5	1½	13	120	4	8½
14	93	5	...	14	94	4	6½
15	114	5	...	15	96	4	7

Among the women the tallest was 4 feet 9 inches, and the shortest 4 feet 3½ inches; giving a mean height for them of 4 feet 6·03 inches. The heaviest woman weighed 126 lbs., and the lightest 85 lbs., giving a mean weight for these of 98·6 lbs.

The men are somewhat athletic and symmetrical in build, with fairly good chests and general muscular development.

The women are very ungainly; they undergo large development about the hips and abdomen. The latter becomes in any woman after 18 years of age very protuberant. This condition, I should think, is induced partly by the complete absence of any proper abdominal support during the period of pregnancy, and is partly due to the distension their stomachs undergo habitually after food. From childhood they cram their stomachs with an immense amount of bulky food in a short period, and they do literally "swell visibly" after their meals. This distended abdominal condition is noticeable in children of both sexes, but as the lads grow up they take more exercise, and their abdominal as well as other muscles become

firmer, and restrain the mechanical distension of the belly. With the women it is different; these latter influences do not exist, and when the child-bearing period comes the abdominal enlargement is augmented. The women also undergo exceptional enlargement in the gluteal region, the appearance of which is enhanced by their custom of attaching branches of twigs to their waist-bands, so as to hang over this region. This large development of abdomen, hip, and buttock, added to a very "waddling" style of gait, gives these women in progression a most grotesque appearance. They mostly possess considerable mammary development, and the glands in many instances seem to be in a chronic state of functional activity. This may be due to the late period to which they suckle their young (even to three and four years), or to another purpose to which the milk is applied, and to which I shall afterwards advert.

The Andamanese are strictly monogamous. The men marry at about eighteen years of age, and the women at any age after twelve, generally about thirteen or fourteen years. The number of children born to each pair is two or three, seldom more; with the women, from seventeen to twenty-five would seem to be the most fruitful period, and it is remarkable how aged the women become in appearance after having had one or two children. Labour with these women is a very simple business. They do their usual work until the actual commencement of the pains, and resume it very shortly after it is over. Two hours seems to be a fair allowance of time for an Andamanese woman to be delivered in.

The thinking powers of these people is not so much deficient as it is limited by their imperfect dialect. Thus, they have no conception of number beyond five, which they count by the fingers of one hand successively touching the nose. As a result they cannot express periods of time, and thus none of them know their own age. The latter can only be guessed at by their physical development and comparison with others of known age. Their ideas and dialect, in the crude state, are very imperfect. They can only reason about, and express ideas on the most tangible objects. Hence it is a matter of considerable difficulty to obtain their ideas on anything else, even by the aid of a fluent interpreter. Their ideas on things abstract are probably none, or are impossible to get at. On the above subject I have been careful to endeavour to get the views of the pure savage,

recently arrived from a remote part of the jungle, and having had no previous intercourse with white people. The Andamanese resident here are of no use for the purpose of ascertaining their primitive mental condition. They all have—especially the lads at school—undergone such a considerable mental enlargement, as I will instance in the case of a boy presently. The following brief statement of their views on some abstract ideas I obtained from a new arrival. Apparently some have no, and others the vaguest, conception of a deity or of departed spirits. By Pullôga they express the power which seems to have arranged natural objects as they now stand. They have a dim idea that some evil spirit or influence (no word to express that abstraction) sends disease among them, and that the latter is especially likely if they go into water after having rubbed on turtle fat! They have a vague fear of going about in the jungle when it is dark. This would seem to point to their recognition of some malign influence which is then liable to assail them. My informants could specialise no further on these matters, either from never having thought about the subject at all, or from want of words to answer such unusual inquiries.

In their dialect identical sounds would seem to have their meaning altered by the tone or key in which they are uttered. The result is that when speaking rapidly and consecutively, the voice modulates up and down in a manner not unlike a person speaking rapidly while humming a chant of three or four notes. The dialects of these islands are remarkable from their variety. The following are some :—

(1.) Bôjengîjîda, or South Andaman (spoken here).
(2.) Balawâda, or Andaman Archipelago.
(3.) Bôjigîâbda, or South Middle Andaman.
(4.) Âkâkôlda, or East Middle Andaman.
(5.) Awkojâwaida, or West Middle Andaman.
(6.) Âkâkêdê, or Interview Island.

Some of these are widely different, and the tribes living within twenty miles can often barely understand each other, those more remote being quite mutually unintelligible.

They are conservative in their language, and generally prefer to invent a new word for a previously unnamed object than to borrow it. A certain number of English and Hindostani words are now

used by them as "chirut" for cheeroot, "kâptan" for captain, "jômôdar" for "jemmadhar," a native policeman, &c.

As an instance of their inventive powers for new names for new objects, I may give—

Bîrmalâkâbangda, a steam-boat.

When asked the name of any, to them, strange objects, they readily extemporise one, as the other day when they were shown a thermometer and at once gave it a name! Doubtless, if the name they gave could be analysed, it would be found to mean "glass-stick," glittering ball, or some such tangible name, as they could form no approximate conception or idea of measuring heat.

As I before mentioned, these people are not deficient in brain-power; it rather lies dormant and unused in their savage state. When I had charge of the Andamanese Hospital, there was a patient in it called Jerry, an Andaman lad of twelve, who had been converted to Christianity and educated in the Ross school. He was of pure Andaman parentage, and had not attained the age of puberty, yet he could read English and Oordoo (Hindustani) fluently, as well as speak and write in both of these languages,—retaining also a knowledge of his Andaman language,—and had a fair knowledge of arithmetic. A number of these lads are being so educated, and the intelligence they display when the latter is developed is considerable.

By disposition these people are generous and affable, always merry and laughing. When one receives a present of tobacco, &c., he always shares it equally with his friends. The same is done with the proceeds of the chase. The men and women never work together. The men do little more than hunt pig, spear turtle, and catch fish. The women do all the remaining work of the community, including the bringing of firewood, and water, making and keeping up the fires, &c. The latter are also the barbers of the community, and it is in this capacity that their milk subserves the other use I before adverted to, viz., the function of shaving soap! It may be seen from the figures that these people all shave the head. When this process requires performing, the person to be shaved sits before the female barber, the latter then compresses her breast with the hand, and directing the nipple over the part to be shaved, emits a jet of milk. This fluid is rubbed in with the finger, and then the surface very cleanly shaved with a piece of broken glass or shell!

Their diet consists mostly of pig and turtle flesh, together with various edible roots, Jack fruit, plantains, mangoes, &c. Those in Hospital or in the Convalescent House have a regular daily ration of curry and rice, properly cooked. They also consume a large amount of sugar-cane, which they are very fond of chewing. I have seen a comparatively small child chew out the sugar from a yard of cane of an inch thick, with the result of visible distension of its stomach at the conclusion of the meal.

The men are good hunters and marksmen with the bow and arrow.

In spearing turtle they use a barbed arrow with removable head. This comes out from the arrow shaft, but is attached by means of a strong cord to the boat containing the hunters; thus the turtles are secured alive and stored up in tanks. A similar plan is adopted in shooting pigs. They are very skilful at shooting fish under water, they seem intuitively to have calculated with great accuracy the difference of direction to be allowed from oblique aqueous refraction, and, I am told, shoot fish in this manner at a distance of thirty yards, with three-pronged barbed arrows.

Their bows are double curve bows of great strength. The bow strings are made by the women, out of the fibrous portion of certain jungle sapplings. This is obtained by picking off the outer bark and scraping it with a shell, which separates the fibrous portion, and the latter is subsequently made into cord by twisting between the hands. Their arrows are long and with variously-shaped heads, according to the use to which they are applied; some are seen in the plate (figs. 2 and 3). I have seen some arrows used in war, with such broad heads that they would completely disembowel an enemy if striking in the abdomen. They are not apparently acquainted with any substance for poisoning their shafts.

In personal adornment they are very curious, and their decorations seem, according to our ideas, very grotesque.

The men, when out hunting, &c., in the jungle wear little or no appendage. They paint and wear more ornaments when returned and living in their community, and of course most of all in connection with any "nautch" or public ceremony. The women are always more adorned. They, however, occasionally put on bracelets and anklets, consisting of simple bands with a number of curled leafy appendages attached thereto. In fig. 4 may be seen cer-

tain necklaces worn by the women mostly. These consist of the bones of their departed friends. The bones most generally used for this decorative purpose are the metacarpal, metatarsal, and the larger phalanges of hand and foot. I have seen a few necklaces in use where the vertebræ are used for this purpose. When the former class of bones are used they detach the articular extremities, and connect the medullary cavities by string as seen in the figure.

Another curious decoration is the attaching of a skull round the neck by string, and allowing it to hang down the back. This is only done with the skulls of chiefs or others who were of importance during their lifetime, and is generally reckoned a mark of esteem to the memory of the person whose skull is so used (fig. 3).

Tatooing the body is much practised by them, and is generally performed about the period of adolescence. It is performed by cutting the skin with sharp pieces of shell or glass, then washing the cut surface with water, then heating the same over a fire, until—I should imagine—it becomes painfully hot, and finally rubbing in a mixture of red clay and turtle fat. When quite healed, the cuts merely present the appearance of so many cicatrices, no especial coloration being retained by them. They also paint their bodies a great deal, and often in the most fantastic patterns, one side white checks, other side red stripes, &c. The colours mostly used are white, red, and a kind of yellow. The former painted all over the face as well as body, indicates that the person so adorned is in mourning for the recent decease of some near relative. The red paint is made by mixing a red (iron oxide) clay found here with turtle fat, and the yellow seems to be a mixture of this with the white, together with some coloured sap from one of the jungle trees. The paint is applied in all sorts of ways, a favourite one with the women being to paint the face white, with eyebrows and nose red!

As I before mentioned, they are a happy and joyous people, always in exuberant spirits. They are very fond of singing and dancing. When they perform a regular national dance the men are the dancers, the women and children forming the accompanying "band and chorus." The latter all sit in one place, and sing in approximate unison a series of notes, which, as far as they can be

represented in our musical notation, are indicated in the annexed staves—

This chant is accompanied by the rhythmical clapping of hands and beating of a resonant piece of wood, of which I have indicated the time in the separate notes above. This rhythmical clapping is kept up all through the performance, even when the singing stops. The dancers form in Indian file, the body slightly bent and the arms overhead, with the hands joined like in the position for diving. They then dance forward, one at a time, with a peculiar tripping step, giving the ground at every second pace a peculiar kind of back kick. The words of the chant are first given out solo by the leading dancer, and then taken up by the women's chorus, and repeated over and over again. They extemporise the words on any passing subject. Thus at one dance, at which I was present, I was the subject of their lay. The words they sang, broadly translated, having the following effect :—

> "Behold the Doctor protector [Mumjôla] (repeated).
> He gives medicine to our sick . . (repeated).
> Their wounds soon close . . . . (repeated).
> They return to hunt in the jungle" . (repeated).

Occasionally they repeat only the first line of the extemporised song.

When one of their number dies the body is taken into the jungle and placed in a small hut in a tree. There it is allowed to remain until the bones have become cleaned by the insects and weather. The bones are then used for ornaments as mentioned.

Explanation of Plate.

Fig. 1. Group of Andamanese men.
,, 2. Shows the mode of using the bow.
,, 3. Two Andamanese men—one wearing a skull on his back.
,, 4. Andamanese women with a bone necklace.

The figures are reproduced from photographs.

3. The Action of Sulphide of Potassium upon Chloroform. By W. W. J. Nicol, M.A. Communicated by Professor Crum Brown.

(*Abstract.*)

The author, after referring to the paper by Pfankuch,* who obtained by the action of sulphide of potassium upon chloroform a crystalline substance which he held to be a compound of sulphide of potassium and sulphoform, and to the paper by Bouchardat† on sulphoform, gives a detailed account of his own investigation.

The products formed when chloroform acts on an alcoholic solution of sulphide of potassium (prepared by dissolving caustic potash in alcohol, saturating one-half with sulphuretted hydrogen and then adding the other half) are sulphydrate of potassium and *thioformiate of potassium* (HCOSK, analogous in constitution to thiacetate). The action is probably as follows:—$HCCl_3 + 2K_2S = 3KCl + HCSSK$, and $HCSSK + H_2O = HCOSK + H_2S$, this sulphuretted hydrogen forming sulphydrate with the sulphide of potassium.

Thioformiate of potassium is converted into formiate by oxide of mercury. The silver salt blackens owing to formation of sulphide of silver, slowly at ordinary temperatures, rapidly on heating.

The free acid could not be prepared, its aqueous solution (prepared by the action of sulphuretted hydrogen on the lead salt suspended in water) rapidly decomposes, yielding formic acid.

4. Note on the Elimination of Linear and Vector Functions. By Professor Tait.

BUSINESS.

Professor Chrystal, George Ritchie Gilruth, L.R.C.S.E., D. Lloyd Roberts, F.R.C.P.L., A. H. Japp, LL.D., and Donald Ross, H.M. Inspector of Schools, were balloted for, and declared duly elected Fellows of the Society.

* Journal für pr. Chem. (2), 6, 99.

† Journal de Pharmacie, xxiii. 12.

Monday, 16th February 1880.

The Right Rev. BISHOP COTTERILL, Vice-President, in the Chair.

The following Communications were read:—

1. On the Geology of the Rocky Mountains. By Professor Geikie.

2. On Comets. By Professor Forbes.

The author commenced by stating that although these researches lead him to believe in the existence of two planets revolving in orbits external to that of Neptune, and although there was a great deal of evidence to show that he had actually determined the elements of the orbits, yet the latter point, being dependent on a coincidence of probabilities only, cannot be considered a certainty until the planets are observed.

The author accepts the theory of cometary orbits which supposes that these bodies, wandering through space, are attracted by the sun into the solar system so as to pursue parabolic orbits, and that some of these, in passing a planet, may have their velocity diminished, in which case they will afterwards describe an ellipse in a definite period.

It has long been known that the greatest distances (aphelion distances) to which comets recede from the sun are grouped into classes. Thus there is a large class of comets whose aphelion distance is about the same as the distance of Jupiter from the sun, and another large class with aphelion distance equal to Neptune's distance. The author has noticed that of the other periodic comets there is a large group of aphelion distances 100 times as far from the sun as the earth is, and another about 300 times. The rest of space is very free from aphelion distances. This is shown by the accompanying table.

At the British Association in 1879 Professor Newton of America proved some important propositions with respect to the introduction

of planets into the solar system. In answer to a question he said that his theory explained why the aphelion distance of a comet is generally about the same as the distance of the planet which rendered its orbit elliptic. The author then publicly stated that there could be no longer a doubt that two planets exist beyond the orbit of Neptune, one about 100 times, the other about 300 times the distance of the earth from the sun, with periods of revolution of about 1000 and 5000 years respectively.

If this be the case, the aphelion positions of a majority of the comets in each group would probably lie in one plane, which would be the plane of the planet's orbit. The analogy of the Jupiter group requires this; for although the orbits of the comets connected with Jupiter have every degree of inclination to the ecliptic, the aphelia of most of them are not far distant from the plane of Jupiter's orbit. Some, of course, might be expected to have been deflected from their original orbits slightly by planetary perturbations, especially those which had been a long time in the solar system.

The author calculated out the seven aphelion positions of comets which are grouped at a distance = 100, determining their latitude and longitude.

The author presented to himself the following problems:—1. Are there a fair number of these aphelion positions lying in one plane passing through the sun? 2. Determine the position of the nodes and inclination of this orbit. 3. Is it possible to imagine a planet, moving with tolerable uniformity, to occupy in the course of a few revolutions the aphelion positions exactly at the aphelion dates? 4. Is the velocity which we must for this reason assume about the same as that of a planet whose distance = 100? What is the present position of the planet? The seven comets of this group are:—

	Date.	Aphelion Distance.	Date of Aphelion.	Calculated Period.	L.
			A.D.	Years.	
I.	1840, iv.	96·7	1668	350	313°
II.	1843, i.	100·0	1655	376	225°
III.	1846	108·2	...	...	...
IV.	1861, i.	110·3	1654	413	139°
V.	1793, ii.	111·0	...	...	...
VI.	1861, ii.	111·2	...	...	...
VII.	1855, ii.	124·2	1608	493	192°

On marking upon a celestial globe the aphelion positions, it was immediately seen that four of them lay exactly upon a great circle, cutting the ecliptic at an angle of 53° and at the longitude 250°. This is the plane of the orbit of the supposed planet. It is remarkable that none of the four aphelion distances of the comets marked above I., II., IV., VII., diverge from this plane by more than 2 or 3 degrees.

The longitudes of these four aphelion distances measured on this plane of the hypothetical planet were then calculated; their values measured from ☊) are given in column L above. It is evidently impossible to suppose that the hypothetical planet could in one revolution, either by direct or retrograde motion, have passed each of the aphelion positions at the aphelion times. It was a matter of some labour to determine whether this could happen even in 2 or 3 revolutions, and a large number of hypotheses were tried. Eventually one was found agreeing well with facts, but involving suppositions as to previous apparitions of comets. According to this theory comet IV. was introduced into the system in 409 A.D., comet I. in 968, comet VII. in 1608, and comet II. in 1655. These are dates when the comets reached their aphelion. A planet moving at the rate of 2·788 years to 1°, or 997 years to a revolution, would, if it was 8° ahead of the comet IV. in 409 A.D., be 9° behind comet I. in 968, 6° behind comet VII. in 1608, and 8° ahead of comet VI. in 1651. These are all very close, provided we have evidence that comets IV. and I. have been seen so often before. Comet I. ought from the known period to have been visible in the years 1490 and 1140. The comet of the previous date has always been suspected to be the same as comet I. The comets which were seen about 1140 are so vaguely described that they cannot be identified.

Comet IV. ought to have been seen in 1861, 1446, 1031, 616 A.D., comets were seen in 1861, 1444, 1032, 617 A.D.

All apparitions previous to 1861 were seen about July, and in each case the star β Leonis is mentioned as being close to the comet. It remained only to see if the orbit of the comet IV. which appeared in 1861 suits these conditions. The author converted the heliocentric elements of the comet's position just before reaching the ecliptic into geocentric elements on the supposition of the earth being in its July position. The result is that the comet would be seen to pass within

a degree of β Leonis. The path would cut the ecliptic with geocentric longitude = 170°.

This will be conclusive evidence to all that these four comets are identical, and this in itself is an interesting deduction from the present research.

The eccentricity and longitude of perihelion of the plane of the planet's orbit can be estimated by noticing the different aphelion distances at different longitudes. This gives us ·08 as the eccentricity and ϖ about 290.°

The period of the supposed planet gives a mean distance = 98, which agrees with what goes before.

The elements of the planet for July 1880 are consequently—

$$a = 98°$$
$$\Omega = 250°$$
$$i = 53°$$
$$\epsilon = \cdot 08°$$
$$\varpi = 290°$$

Motion direct.

Distance of planet from node = 296°.

Its present longitude measured from Ω up to the node and then along the planet's orbit is 185°.

[*Additional Note*, 31*st March* 1880.—Another calculation was made on the supposition of the planet being on the plane of the ecliptic, and affecting the comets when nearest to their aphelion positions. This gives its present longitude = 184°.

This hypothesis is strongly supported by the fact that the author has computed the position of Neptune by its influence on comets correct within 2°, although previously ignorant of its position.]

From the six comets whose aphelion distance is about 300 times the distance of the earth from the sun, the elements of the perturbing planet have roughly been calculated. This gives—

$$\Omega = 185°$$
$$i = 45°$$

nearly the same orbit as the preceding. The present position is—

R. A. 22h. 0m.
N. Declination 39°.

In the neighbourhood of this position, but at a position occupied by

the planet 100 years ago, was a star "11 Vulpeculæ," which Baily reported missing. The best orbit to suit the position of that star is—

$$\Omega = 262^\circ$$
$$i = 55^\circ.$$

If this be the true orbit the present position must be slightly altered.

Those comets which have been in the solar system for an enormous time would have their orbits probably deflected, and would not therefore appear in these calculations.

3. Note on the Velocity of Gaseous Particles at the Negative Pole of a Vacuum Tube. By Professor Tait.

The recent exhibition by Professor G. Forbes of some of the latest of Mr Crookes' experiments, together with what I had read or heard about their explanation, led me to infer that I might determine directly the velocity of the luminous particles near the negative pole (and perhaps at other parts) of a vacuum tube by means of observations of the spectrum made in directions perpendicular to, and parallel to, the lines of motion of the incandescent particles of gas.

I made the attempt on some charcoal-bromine vacuum tubes, for which I have to thank Professor Dewar, but I found the light to be so feeble that it was impossible to employ an eight-prism spectroscope. A one-prism spectroscope, when the spectra taken in and perpendicular to the direction of motion of the particles were placed side by side, showed merely that the velocity could not amount to anything like 90 miles per second. There did seem to be a very slight shifting of the former spectrum towards the violet, but this appearance was probably due to the fact that its light had been weakened by two reflections, while that of the other was taken direct.

It was evident that one cause at least of the failure is the great loss of light by multiplied reflections when a powerful spectroscope is employed. Thus I was driven to try the only other available method with which I was acquainted, and which indeed I had employed for more than ten years as an occasional part of the routine work in the physical laboratory. This method depends upon rotation (by quartz) of the plane of polarisation, combined (when necessary)

with sufficient prism dispersion just to separate the various bright lines of the source from one another.

My former assistant, Professor D. H. Marshall, made for me, in 1870, a series of careful measurements of the change of plane of polarisation of the lines C and F of hydrogen by this method, using a vacuum tube with a narrow bore, no slit, and a prism of small angle. It was found to give fair but not excellent results. Although no greater thickness of quartz was employed than the plates supplied along with Duboscq's saccharimeter, the planes of polarisation of C and F were separated by the thickest of them upwards of 130°; but the determination of the exact point of extinction is not easy. In measuring with practically homogeneous light, like that of a spirit-lamp with chloride of sodium or of lithium, the *prismatic* dispersion was, of course, not required. The great merit of the rotatory polarisation process consists in the fact that there is scarcely any additional loss of light incurred by using a foot or two of quartz instead of a few millimetres, and thus in proportion increasing the amount of rotatory displacement; while the thicker the quartz the less is the inevitable percentage error of observation. Also the position of each bright line is determined in terms of a standard quartz-rotation, and needs no comparison spectrum. It remains to be seen whether, on trial, it may be found possible to have a great length of quartz cut with sufficient accuracy, and whether the bright lines are narrow enough for this mode of observation. I have ordered a 6-inch cylinder of quartz, and hope soon to have observations made with it. Meanwhile it seems likely that this combination of polarising and analysing prisms, with a quartz plate, and a small direct vision spectroscope (with very wide slit), may be well adapted for measurements of position of the bright lines in the spectra of auroras, comets, and nebulæ, where it is not easy to employ either a comparison spectrum or a wire micrometer.

Monday, 1st March 1880.

Professor GEIKIE in the Chair.

The following Communications were read:—

1. On Steam-Pressure Thermometers of Sulphurous Acid, Water, and Mercury. By Sir W. Thomson.

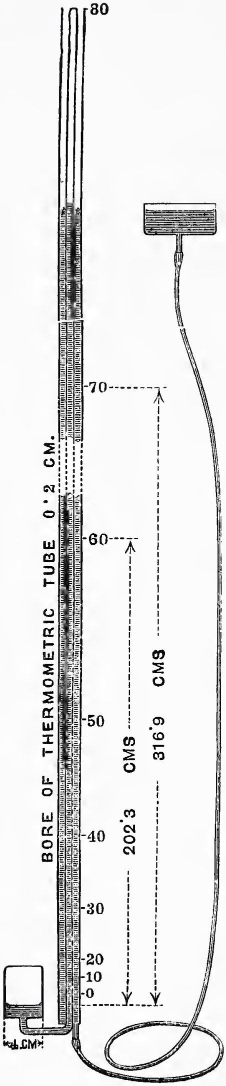


Fig. 1.

The first annexed diagram represents a thermometer constructed to show absolute temperature realised for the case of water and vapour of water as thermometric substance. The containing vessel consists of a tube with cylindric bulb like an ordinary thermometer; but, unlike an ordinary thermometer, the tube is bent in the manner shown in the drawing. The tube may be of from 1 to 2 or 3 millims. bore, and the cylindrical part of the bulb of about ten times as much. The length of the cylindrical part of the bulb may be rather more than $\frac{1}{100}$ of the length of the straight part of the tube. The contents, water and vapour of water, are to be put in and the glass hermetically sealed to enclose them, with the utmost precautions to obtain pure water as thoroughly freed from air as possible, after better than the best manner of instrument makers in making cryophoruses and water hammers. The quantity of water left in at the sealing must be enough to fill the cylindrical part of the bulb and the horizontal branch of the tube. When in use the straight part of the

tube must be vertical with its closed end up, and the part of it occupied by the manometric water-column must be kept at a nearly enough definite temperature by a surrounding glass jacket-tube of ice-water. This glass jacket-tube is wide enough to allow little lumps of ice to be dropped into it from its upper end, which is open. By aid of an india-rubber tube connected with its lower end, and a little movable cistern, as shown in the drawing, the level of the water in the jacket is kept from a few inches above to a quarter of an inch below that of the interior manometric column. Thus, by dropping in lumps of ice so as always to keep some unmelted ice floating in the water of the jacket, it is easy to keep the temperature of the top of the manometric water-column exactly at the freezing temperature. As we shall see presently, the manometric water below its free surface may be at any temperature from freezing to 10° C. above freezing without more than $\frac{1}{40}$ per cent. of hydrostatic error. The temperature in the vapour-space above the liquid column may be either freezing or anything higher. It ought not to be lower than freezing, because, if it were so, vapour would condense as hoar frost on the glass, and evaporation from the top of the liquid column would either cryophoruswise freeze the liquid there, or cool it below the freezing point.

The chief object of keeping the top of the manometric column exactly at the freezing-point is to render perfectly definite and constant the steam-pressure in the space above it.

A second object of considerable importance when the bore of the tube is so small as one millimetre, is to give constancy to the capillary tension of the surface of the water. The elevation by capillary attraction of ice-cold water in a tube of one millimetre bore is about 7 millims. The constancy of temperature provided by the surrounding iced water will be more than sufficient to prevent any perceptible error due to inequality of this effect. To avoid error from capillary attraction the bore of the tube ought to be very uniform, if it is so small as one millimetre. If it be three millimetres or more, a very rough approach to uniformity would suffice.

A third object of the iced-water jacket, and one of much more importance than the second, is to give accuracy to the hydrostatic measurement by keeping the density of the water throughout the long vertical branch definite and constant. But the density of water

at the freezing point is only $\frac{1}{40}$ per cent. less than the maximum density, and is the same as the density at 8° C.; and therefore when $\frac{1}{40}$ per cent. is an admissible error on our thermometric pressure, the density will be nearly enough constant with any temperature from 0° to 10° C. throughout the column. But on account of the first object mentioned above, the very top of the water-column must be kept with exceeding exactness at the freezing temperature.

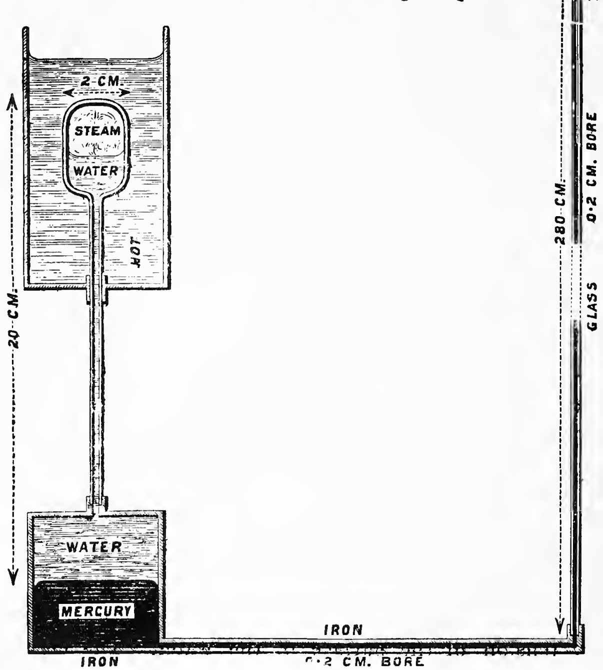


Fig. 2.

In this instrument the "thermometric substance" is the water and vapour of water in the bulb, or more properly speaking the portions of water and vapour of water infinitely near their separating interface. The rest of the water is merely a means of measuring hydrostatically the fluid pressure at the interface. When the temperature is so high as to make the pressure too great to be conveniently measured by a water column, the hydrostatic measurement may be done, as shown in the second annexed drawing (fig. 2), by a mercury column in a glass tube, surrounded by a glass water jacket not shown in the drawing, to keep it very accurately at some

definite temperature so that the density of the mercury may be accurately known.

The simple form of steam thermometer represented with figured dimensions in the first diagram will be very convenient for practical use for temperatures from freezing to 60°. Through this range the pressure of vapour of water, reckoned in terms of the balancing column of water of maximum density, increases from $6\frac{1}{4}$ to 202·4 centimetres; and for this, therefore, a tube of a little more than 2 metres will suffice. From 60° to 140° the pressure of steam now reckoned in terms of the length of a balancing column of mercury at 0° increases from 14·88 to 271·8 centimetres; and for this a tube of 280 centimetres may be provided. For higher temperatures a longer column, or several columns, as in the multiple manometer, or an accurate air pressure-gauge, or some other means, such as a very accurate instrument constructed on the principle of Bourdon's metallic pressure-gauge, may be employed, so as to allow us still to use water and vapour of water as thermometric substance.

High-pressure Steam Thermometer.

At 230° C., the superior limit of Regnault's high-pressure steam experiments, the pressure is 27·53 atmos, but there is no need for limiting our steam thermometer to this temperature and pressure. Suitable means can easily be found for measuring with all needful accuracy much higher pressures than 27 atmos. But at so high a temperature as 140°, vapour of mercury measured by a water column, as shown in the diagram (fig. 3), becomes available for purposes for which one millimetre to the degree is a sufficient sensibility. The mercury-steam-pressure thermometer, with pressure measured by water-column, of dimensions shown in the drawing, serves from 140° to 280° C., and will have very ample sensibility through the upper half of its scale. At 280° its sensibility will be about $4\frac{3}{4}$ centimetres to the degree! For temperatures above 280° sufficient sensibility for most purposes is obtained by substituting mercury for water in that simplest form of steam thermometer shown in fig. 1, in which the pressure of the steam is measured by a column of the liquid itself kept at a definite temperature. When the liquid is mercury there is no virtue in the parti-

cular temperature 0° C., and a stream of water as nearly as may be of atmospheric temperature will be the easiest as well as the most accurate way of keeping the mercury at a definite temperature. As the pressure of mercury steam is at all ordinary atmospheric temperatures quite imperceptible to the hydrostatic test when mercury itself is the balancing liquid, that which was the chief reason for fixing the temperature at the interface between liquid and vapour at the top of the pressure-measuring column when the balancing liquid was water, has no weight in the present case; but, on the other hand, a much more precise definiteness than the ten degrees latitude allowed in the former case for the temperature of the main length of the manometric column

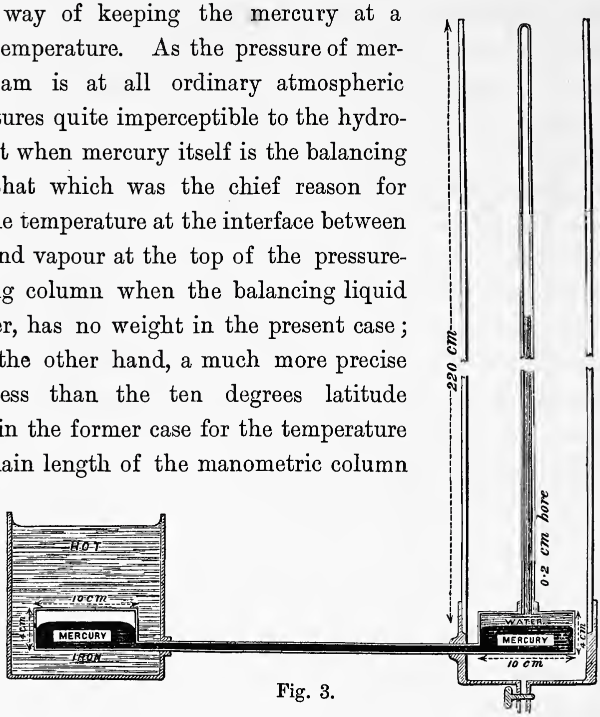


Fig. 3.

is now necessary. In fact, a change of temperature of 2·2° in mercury at any atmospheric temperature produces about the same proportionate change of density as is produced in water by a change of temperature from 0° to 10°, that is to say, about $\frac{1}{20}$ per cent.; but there is no difficulty in keeping, by means of a water jacket, the mercury column constant to some definite temperature within a vastly smaller margin of error than 2·2°, especially if we choose for the definite temperature something near the atmospheric temperature at the time, or the temperature of whatever abundant water supply may be available. If the glass tube for the pressure-measuring mercury column be 838 centimeters long, the simple mercury-steam thermometer may be used up to 520° C., the highest temperature reached by Regnault in his experiments on mercury-steam. By using an iron bulb and tube for the part of the thermometer exposed to the high

temperature, and for the lower part of the measuring column to within a few metres of its top, with glass for the upper part to allow the mercury to be seen, a mercury-steam-pressure thermometer can with great ease be made which shall be applicable for temperatures giving pressures up to as many atmospheres as can be measured by the vertical height available. The apparatus may of course be simplified by dispensing with the Torricellian vacuum at the upper end of the tube, and opening the tube to the atmosphere, when the steam-pressure to be measured is so great that a rough and easy barometer observation gives with sufficient accuracy the air-pressure at the top of the measuring column. The easiest, and not necessarily in practice the least accurate, way of measuring very high pressures of mercury-steam will be by enclosing some air above the cool, pressure-measuring column of mercury, and so making it into a compressed-air pressure-gauge, it being understood that the law of compression of the air under the pressures for which it is to be used in the gauge is known by accurate independent experiments such as those of Regnault on the compressibility of air and other gases.

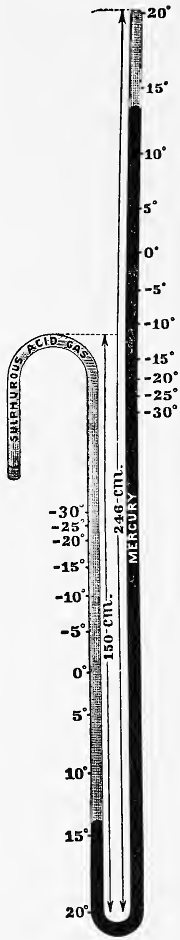


Fig. 4.

The water-steam thermometer may be used, but somewhat precariously, for temperatures below the freezing-point, because water, especially when enclosed and protected as the portion of it in the bulb of our thermometer is, may be cooled many degrees below its freezing-point without becoming frozen; but, not to speak of the uncertainty or instability of this peculiar condition of water, the instrument would be unsatisfactory on account of insufficient thermometric sensibility for temperatures more than two or three degrees below the freezing-point. Hence, to make a steam thermometer for such temperatures some other substance than water should be taken, and none seems better adapted for the purpose than sulphurous acid, which, in the apparatus represented with

figured dimensions in the accompanying diagram (fig. 4), makes an admirably convenient and sensitive thermometer for temperatures from $+20°$ to something far below $-30°$, as we see from the results of Regnault's measurements.

To sum up, we have, in the preceding description and drawings, a complete series of steam-pressure thermometers, of sulphurous acid, of water, and of mercury, adapted to give absolutely definite and highly sensitive thermometric indications throughout the wide range from something much below $-30°$ to considerably above $520°$ of the centigrade scale. The graduation of the scales of these thermometers to show absolute temperature is to be made by calculation from the thermodynamic formula—

$$t = t_0 \epsilon^{\int_{p_0}^{p} \frac{(1-\sigma)dp}{J\rho\kappa}}$$

where t denotes the absolute temperature corresponding to steam-pressure p; p_0 to the absolute temperature corresponding to steam-pressure p_0; κ the latent heat of the steam per unit mass; ρ the density of the steam; and σ the ratio of the density of the steam to the density of the liquid in contact with it. When the requisite experimental data, that is to say, the values of σ and $\rho\kappa$ for different values of p throughout the range for which each substance is to be used as thermometric fluid are available, the graduation of the scales of these thermometers to show absolute temperature can be performed in practice by calculation from the formula. Hitherto these requisites have not been all given by direct experiment for any one of the three substances with sufficient accuracy for our thermometric purpose through any range whatever. Water, naturally, is the one for which the nearest approach to the requisite information has been obtained. For it Regnault's experiments have given, no doubt with great accuracy, the values of p and of κ for all temperatures reckoned by his normal air thermometer, which we now regard merely as an arbitrary scale of temperature, through the range from $-30°$ to $+230°$. If he, or any other experimenter, had given us with similar accuracy through the same range the values of ρ and σ for temperatures reckoned on the same arbitrary scale, we should have all the data from experiment required for the graduation of our water-steam thermometer to absolute thermodynamic scale.

For it is to be remarked that all reckoning of temperature is eliminated from the second member of the formula, and that, in our use of it, Regnault's normal thermometer has merely been referred to for the values of $\rho\kappa$ and of $1 - \sigma$, which correspond to stated values of p. The arbitrary constant of integration, t_0, is truly arbitrary. It will be convenient to give it such a value that the difference of values of t between the freezing-point of water and the temperature for which p is equal to one atmo shall be 100, as this makes it agree with the centigrade scale in respect to the difference between the numbers measuring the temperatures which on the centigrade scale are marked 0° and 100°. Indirectly, by means of experiments on hydrogen gas, this assignation of the arbitrary constant of integration would give 273 for the absolute temperature 0° C., and 373 for that of 100° C, as is proved in p. 56 of the article on "Heat," in the *Encyclopædia Britannica.* Meantime, as said above, we have not the complete data from direct experiments even on water-steam for graduating the water-steam thermometer; but, on the other hand we have, from experiments on air and on hydrogen and other gases, data which allow us to graduate indirectly any continuous intrinsic thermoscope according to the absolute scale. By thus indirectly graduating the water-steam thermometer, we learn the density of steam at different temperatures with more probable accuracy than it has hitherto been made known by any direct experiments on water-steam itself.

Merely viewed as a continuous intrinsic thermoscope, the steam-thermometer, in one or other of the forms described above to suit different parts of the entire range from the lowest temperatures to temperatures somewhat above 520°, is no doubt superior in the conditions requisite for accuracy to every other thermoscope of any of the different kinds hitherto in use; and it may be trusted more surely for accuracy than any other as a thermometric standard when once it has been graduated according to the absolute scale, whether by practical experiments on steam, or indirectly by experiments on air or other gases. In fact, the use of steam-pressure measured in definite units of pressure, as a thermoscopic effect, in the steam thermometer is simply a continuous extension to every temperature, of the principle already practically adopted for fixing the temperature which is called 100° on the centigrade scale; and

it stands on precisely the same theoretical footing as an air thermometer, or a mercury-in-glass thermometer, or an alcohol thermometer, or a methyl-butyrate thermometer, in respect to the graduation of its scale according to absolute temperature. Any one intrinsic thermoscope may be so graduated ideally by thermodynamic experiments on the substance itself without the aid of any other thermometer or any other thermometric substance; but the steam-pressure thermometer has the great practical advantage over all others, except the air thermometer, that these experiments are easily realisable with great accuracy instead of being, though ideally possible, hardly to be considered possible as a practical means of attaining to thermodynamic thermometry. In fact, for water-steam it is only the most easily obtained of experimental data, the measurement of the density of the steam at different pressures, that has not already been actually obtained by direct experiment. Whether or not when this lacuna has been filled up by direct experiments, the data from water-steam alone may yield more accurate thermodynamic thermometry than we have at present from the hydrogen or nitrogen gas thermometer,—to be described in a subsequent communication to the Royal Society (Proceedings, April 19, 1880)—we are unable at present to judge. But when once we have the means, directly from itself, or indirectly from comparison with hydrogen or nitrogen or air thermometers, of graduating once for all a sulphurous acid steam thermometer, water-steam thermometer, or mercury-steam thermometer, that is to say, when once we have a table of the absolute thermodynamic temperatures corresponding to the different steam-pressures of the substances sulphurous acid, water, and mercury, we have a much more accurate and more easily reproducible standard than either the air or gas thermometer of any form, or the mercury thermometer, or any liquid thermometer can give. In fact, the series of steam thermometers for the whole range from the lowest temperatures can be reproduced with the greatest ease in any part of the world by a person commencing with no other material than a piece of sulphur and air to burn it in,[1] some pure water, some pure mercury, and

[1] Practically, the best ordinary chemical means of preparing sulphurous acid, as from sulphuric acid, by heating with copper, might be adopted in preference to burning sulphur.

abundance of ice and salt to make freezing mixtures; and with no other apparatus than can be made by a moderately skilled glass-blower; and with no other standard of physical measurement of any kind than an accurate linear measure. He may assume the force of gravity to be that calculated for his latitude with the ordinary rough allowance for his elevation above the sea, and his omission to measure with higher accuracy the actual force of gravity in his locality can lead him into no thermometric error which is not incomparably less than the inevitable errors in the reproduction and use of the air thermometer, or of mercury or other liquid thermometers. In temperatures above the highest for which mercury-steam pressure is not too great to be practically available, nothing hitherto invented but Deville's air thermometer with hard porcelain bulb suited to resist the high temperature is available for accurate thermometry.

The following statement is in the *Encyclopædia Britannica* article "Heat," appended to the description of steam-pressure thermometers which it contains:—"We have given the steam thermometer as our first example of thermodynamic thermometry because intelligence in thermodynamics has been hitherto much retarded, and the student unnecessarily perplexed, and a mere quicksand has been given as a foundation for thermometry, by building from the beginning on an ideal substance called perfect gas, with none of its properties realised rigorously by any real substance, and with some of them unknown, and utterly unassignable, even by guess. But after having been moved by this reason to give the steam-pressure thermometer as our first theoretical example, we have been led into the preceding carefully detailed examination of its practical qualities, and we have thus become convinced that though hitherto used in scientific investigations only for fixing the "boiling-point," and (through an inevitable natural selection) by practical engineers for knowing the temperatures of their boilers by the pressures indicated by the Bourdon's gauge, it is destined to be of great service both in the strictest scientific thermometry, and as a practical thermometer for a great variety of useful applications."

2. On a Sulphurous Acid Cryophorus. By Sir W. Thomson.

(*Abstract.*)

The instrument exhibited to the Royal Society consisted of a U-shaped glass tube stopped at both ends, containing sulphurous acid liquid and steam. The process by which the sulphurous acid is freed from air, which was partially exhibited to the Royal Society, is as follows:—

Begin with a glass U tube open at both ends, and attach to each a small convenient, very fine, and perfectly gas-tight, stop-cock. Placing it with the bend down in a freezing mixture, condense pure well-dried sulphurous acid gas direct into it from the generator till it is full nearly to the tops of the two branches. Then close the stop-cock, detach from the generator, and remove from the freezing mixture. Holding it still with the bend down, apply gentle heat to the bend, by a warm hand or by aid of a spirit-lamp, so as to produce boiling, the bubbles rising up in either one or the other of the two branches. After doing this for some time let the bend cool, and apply gentle heat to the surface of the liquid in that one of the branches into which the bubbles passed. With great care now open very slightly the stop-cock at the top of this branch, until the liquid is up to very near the top of the tube, and close the stop-cock before it begins to blow out. Repeat the process several times, causing the bubbles sometimes to rise up one branch, and sometimes up the other. After this has been done two or three dozen times, it is quite certain that only a very infinitesimal amount of air can have remained in the apparatus. When satisfied that this is the case, sink the bend once more into a freezing mixture, and with a convenient blow-pipe and flame melt the glass tube below each stop-cock so as to hermetically seal the two ends of the U tube, and detach them from the stop-cocks. This completes the construction of the sulphurous acid cryophorus.

The instrument, if turned with the bend up and the two sealed ends down, may be used as a cryophorus presenting interesting peculiarities.

The most interesting qualities are those which it presents when

held with the bend down. In this position it constitutes a differential thermometer of exceedingly high sensibility, founded on the difference of sulphurous acid steam-pressure due to difference of pressure in the two branches. One very remarkable and interesting feature is the exceeding sluggishness with which the liquid finds its level in the two branches when the external temperature is absolutely uniform all round. In this respect it presents a most remarkable contrast with a U tube, in other respects similar, but occupied by water and water-steam instead of sulphurous acid and sulphurous-acid-steam. If the U tube of water be suddenly inclined 10 or 20 degrees to the vertical in the plane of the two branches, the water oscillates before it settles with the free surfaces in the two branches at the same level. When the same is done to the U tube of sulphurous acid, it seems to take no notice of gravity; but in the course of several minutes it is seen that the liquid is sinking slowly in one branch and rising in the other towards identity of level. The reason is obvious.

3. Vibrations of a Columnar Vortex.

By Sir William Thomson.

This is a case of fluid motion, in which the stream lines are approximately circles, with their centres in one line (the axis of the vortex) and the velocities approximately constant, and approximately equal at equal distances from the axis. As a preliminary to treating it, it is convenient to express the equations of motion of a homogeneous incompressible inviscid fluid (the description of fluid to which the present investigation is confined) in terms of "columnar co-ordinates" r, θ, z, that is co-ordinates such that $r \cos \theta = x$, $r \sin \theta = y$.

If we call the density unity, and if we denote by $\dot{x}, \dot{y}, \dot{z}$ the velocity-components of the fluid particle which at time t is passing through the point (x, y, z); and by $\frac{d}{dt}, \frac{d}{dx}, \frac{d}{dy}, \frac{d}{dz}$ differentiations respectively on the supposition of x, y, z constant, t, y, z constant, t, x, z constant, and t, x, y constant, the ordinary equations of motion are

$$\left.\begin{aligned}-\frac{dp}{dx}&=\frac{d\dot{x}}{dt}+\dot{x}\frac{d\dot{x}}{dx}+\dot{y}\frac{d\dot{x}}{dy}+\dot{z}\frac{d\dot{x}}{dz}\\-\frac{dp}{dy}&=\frac{d\dot{y}}{dt}+\dot{x}\frac{d\dot{y}}{dx}+\dot{y}\frac{d\dot{y}}{dy}+\dot{z}\frac{d\dot{y}}{dz}\\-\frac{dp}{dz}&=\frac{d\dot{z}}{dt}+\dot{x}\frac{d\dot{z}}{dx}+\dot{y}\frac{d\dot{z}}{dy}+\dot{z}\frac{d\dot{z}}{dz}\end{aligned}\right\}\quad . \quad . \quad . \quad (1),$$

and

$$\frac{d\dot{x}}{dx}+\frac{d\dot{y}}{dy}+\frac{d\dot{z}}{dz}=0\quad . \quad . \quad . \quad (2).$$

To transform to the columnar co-ordinates we have

$$\left.\begin{aligned}x&=r\cos\theta,\ y=r\sin\theta\\\dot{x}&=\dot{r}\cos\theta-r\dot{\theta}\sin\theta\\\dot{y}&=\dot{r}\sin\theta+r\dot{\theta}\cos\theta\\\frac{d}{dx}&=\cos\theta\frac{d}{dr}-\sin\theta\frac{d}{rd\theta}\\\frac{d}{dy}&=\sin\theta\frac{d}{dr}+\cos\theta\frac{d}{rd\theta}\end{aligned}\right\}\quad . \quad . \quad (3),$$

The transformed equations are

$$\left.\begin{aligned}-\frac{dp}{dr}&=\frac{d\dot{r}}{dt}+\dot{r}\frac{d\dot{r}}{dr}-\frac{(r\dot{\theta})^2}{r}+\dot{\theta}\frac{d\dot{r}}{d\theta}+\dot{z}\frac{d\dot{r}}{dz}\\-\frac{dp}{rd\theta}&=r\frac{d\dot{\theta}}{dt}+\dot{r}\frac{d(r\dot{\theta})}{dr}+\dot{r}\dot{\theta}+\dot{\theta}\frac{d(r\dot{\theta})}{d\theta}+\dot{z}\frac{d(r\dot{\theta})}{dz}\\-\frac{dp}{dz}&=\frac{d\dot{z}}{dt}+\dot{r}\frac{d\dot{z}}{dr}+\dot{\theta}\frac{d\dot{z}}{d\theta}+\dot{z}\frac{d\dot{z}}{dz}\end{aligned}\right\}\quad . \quad (4),$$

and

$$\frac{d\dot{r}}{dr}+\frac{\dot{r}}{r}+\frac{d(r\dot{\theta})}{rdr}+\frac{d\dot{z}}{dz}=0\quad . \quad . \quad . \quad (5).$$

Now let the motion be approximately in circles round $0z$, with velocity everywhere approximately equal to T, a function of r; and to fulfil these conditions assume

$$\left.\begin{aligned}&\dot{r}=\varrho\cos mz\sin(nt-i\theta);\quad r\dot{\theta}=\mathrm{T}+\tau\cos mz\cos(nt-i\theta)\\&\dot{z}=w\sin mz\sin(nt-i\theta);\quad p=\mathrm{P}+\varpi\cos mz\cos(nt-i\theta)\\&\qquad\qquad\text{with } \mathrm{P}=\int\frac{\mathrm{T}^2dr}{r}\end{aligned}\right\}\ (6);$$

where ϱ, τ, w, and ϖ are functions of r, each infinitely small, in

comparison with T. Substituting in (4) and (5) and neglecting squares and products of the infinitely small quantities we find,

$$\left.\begin{aligned} -\frac{d\varpi}{dr} &= \left(n - i\frac{\mathrm{T}}{r}\right)\varrho - 2\frac{\mathrm{T}}{r}\tau \\ -\frac{i\varpi}{r} &= -\left(n - i\frac{\mathrm{T}}{r}\right)\tau + \left(\frac{\mathrm{T}}{r} + \frac{d\mathrm{T}}{dr}\right)\varrho \\ + m\varpi &= \left(n - i\frac{\mathrm{T}}{r}\right)w \end{aligned}\right\} \quad . \quad . \quad (7),$$

$$\frac{d\varrho}{dr} + \frac{\varrho}{r} + \frac{i\tau}{r} + mw = 0 \quad . \quad . \quad . \quad . \quad (8).$$

Taking (7), eliminating ϖ, and resolving for ϱ, τ, we find

$$\left.\begin{aligned} \varrho &= \frac{1}{m\mathrm{D}}\left(n - i\frac{\mathrm{T}}{r}\right)\left\{\left(n - i\frac{\mathrm{T}}{r}\right)\frac{dw}{dr} - \frac{i}{r}\left(\frac{\mathrm{T}}{r} + \frac{d\mathrm{T}}{dr}\right)\frac{w}{r}\right\} \\ \tau &= \frac{1}{m\mathrm{D}}\left\{\left(\frac{\mathrm{T}}{r} + \frac{d\mathrm{T}}{dr}\right)\left(n - i\frac{\mathrm{T}}{r}\right)\frac{dw}{dr} + \frac{i}{r}\left[\frac{\mathrm{T}^2}{r^2} - \frac{d\mathrm{T}^2}{dr^2} - \left(n - i\frac{\mathrm{T}}{r}\right)^2\right]w\right\} \\ \text{where} \quad \mathrm{D} &= \frac{2\mathrm{T}}{r}\left(\frac{\mathrm{T}}{r} + \frac{d\mathrm{T}}{dr}\right) - \left(n - i\frac{\mathrm{T}}{r}\right)^2 \end{aligned}\right\} (9).$$

For the particular case of $m = 0$, or motion in two dimensions (r, θ), it is convenient to put

$$\frac{-w}{m} = \phi \quad . \quad . \quad . \quad . \quad (10).$$

In this case the motion which superimposed on $\dot{r} = 0$ and $r\dot{\theta} = \mathrm{T}$ gives the disturbed motion is irrotational, and $\phi \sin(nt - i\theta)$ is its velocity-potential. It is also to be remarked that when m does not vanish the superimposed motion is irrotational where if at all, and only where, $\mathrm{T} = \text{const.}/r$, and that whenever it is irrotational ϕ as given by (10) is its velocity potential.

Eliminating ϱ and τ from (8) by (9) we have a linear differential equation of the second order for w. The integration of this, and substitutions of the result in (9), give w, ϱ, and τ, in terms of r and the two arbitrary constants of integration which, with m, n, and i, are to be determined to fulfil whatever surface conditions, or initial conditions, or conditions of maintenance, are prescribed for any particular problem.

Crowds of exceedingly interesting cases present themselves. Taking one of the simplest to begin :—

Case I.

Let $$\mathrm{T} = \omega r \ (\omega \text{ const.}) \qquad . \qquad . \qquad (11),$$

$$\left.\begin{array}{l} \dot{r} = c \cos mz \sin (nt - i\theta), \text{ where } r = a \\ \dot{r} = \mathfrak{c} \cos mz \sin (nt - i\theta), \quad \text{,,} \quad r = \mathfrak{a} \\ c, \mathfrak{c}, m, n, a, a' \text{ being any given quantities} \\ \quad \text{and } i \text{ any given integer} \end{array}\right\} \quad . \quad (12).$$

The condition $\mathrm{T} = \omega r$ simplifies (9) to

$$\left.\begin{array}{l} \varrho = \dfrac{(n - i\omega)\left\{\left(n - i\omega\right)\dfrac{dw}{dr} - \dfrac{2i\omega}{r}w\right\}}{m\{4\omega^2 - (n - i\omega)^2\}} \\[2ex] \tau = \dfrac{(n - i\omega)\left\{2\omega\dfrac{dw}{dr} - \dfrac{i(n - i\omega)}{r}w\right\}}{m\{4\omega^2 - (n - i\omega)^2\}} \end{array}\right\} \quad . \quad (13),$$

and the elimination of ϱ and τ by these from (8) gives

$$-\frac{d^2w}{dr^2} + \frac{1}{r}\frac{dw}{dr} - \frac{i^2w}{r^2} + m^2\frac{4\omega^2 - (n - i\omega)^2}{(n - i\omega)^2}w = 0 \quad . \quad (14),$$

or
$$\left.\begin{array}{l} \dfrac{d^2w}{dr^2} + \dfrac{1}{r}\dfrac{dw}{dr} - \dfrac{i^2w}{r^2} + \nu^2 w = 0 \\[2ex] \nu = m\sqrt{\dfrac{4\omega^2 - (n - i\omega)^2}{(n - i\omega)^2}} \end{array}\right\} \quad . \quad . \quad (15),$$
where

or
$$\left.\begin{array}{l} \dfrac{d^2w}{dr^2} + \dfrac{1}{r}\dfrac{dw}{dr} - \dfrac{i^2w}{r^2} - \sigma^2 w = 0 \\[2ex] \sigma = m\sqrt{\dfrac{(n - i\omega)^2 - 4\omega^2}{(n - i\omega)^2}} \end{array}\right\} \quad . \quad . \quad (16).$$
where

Hence if J_i, $\mathfrak{J}_i$ denote Bessel's functions of order i, and of the first and second kinds,* that is to say J_i finite or zero for infinitely small values of r, and $\mathfrak{J}^i$ finite or zero for infinitely great values of r; and if I_i and $\mathfrak{I}_i$ denote the corresponding real functions with ν imaginary, we have .

$$w = \mathrm{C}\mathrm{J}_i(\nu r) + \mathfrak{C}\mathfrak{J}_i(\nu r) \quad . \quad . \quad . \quad (17),$$

* Compare Proceedings, March 17, 1879, "Gravitational Oscillations of Rotating Water." Solution II. (Case of Circular Basons).

or
$$w = \mathrm{CI}_i(\sigma r) + \mathfrak{C}\mathfrak{I}_i(\sigma r) \qquad (18);$$

where C and $\mathfrak{C}$ denote arbitrary constants, to be determined in the present case by the equations of condition (12). These are equivalent to $\varrho = c$ when $r = a$, and $\varrho = \mathfrak{c}$ when $r = \mathfrak{a}$, and, when (16) is used for w in (13), give two simple equations to determine C and $\mathfrak{C}$.

The problem thus solved is the finding of the periodic disturbance in the motion of rotating liquid in a space between two boundaries which are concentric circular cylindric when undisturbed, produced by infinitely small simple harmonic normal motion of these boundaries distributed over them according to the simple harmonic law in respect to the co-ordinates z, θ. The most interesting Sub-case is had by supposing the inner boundary evanescent ($\mathfrak{a} = 0$), and the liquid continuous and undisturbed throughout the space contained by the outer cylindric boundary of radius a. This, as is easily seen, makes $w = 0$ when $r = 0$, except for the case $i = 1$, and essentially, without exception, requires that $\mathfrak{c}$ be zero. Thus the solution for w becomes

$$w = \mathrm{CJ}_i(\nu r) \quad . \quad . \quad . \quad (19)$$
or
$$w = \mathrm{CI}\ (\sigma r) \quad . \quad . \quad . \quad (20);$$

and the condition $\varrho = c$ when $r = a$ gives, by (13),

$$\mathrm{C} = \frac{\nu^2 m}{\nu \mathrm{J}'_i(\nu a) - \dfrac{2i\omega}{(n - i\omega)a}\mathrm{J}_i(\nu a)} \quad . \quad . \quad (21).$$

or the corresponding I formula.

By summation after the manner of Fourier we find the solution for any arbitrary distribution of the generative disturbance over the cylindric surface (or over each of the two if we do not confine ourselves to the Sub-case), and for any arbitrary periodic function of the time. It is to be remarked that (6) represents an undulation travelling round the cylinder with linear velocity na/i at the surface, or angular velocity n/i throughout. To find the interior effect of a *standing* vibration produced at the surface we must add to the solution (6), or any sum of solutions of the same type, a solution, or a sum of solutions in all respects the same, except with $-n$ in place of n.

It is also to be remarked that great enough values of i make ν^2

negative, and therefore ν imaginary; and for such the solutions in terms of σ and the I_i, $\mathfrak{I}_i$ functions must be used.

Case II.—Hollow Irrotational Vortex in a fixed Cylindric Tube.

Conditions—

$$\left.\begin{array}{ll} & \mathrm{T}=\frac{c}{r};\ \dot{r}=0 \text{ when } r=a; \\ \text{and} & \mathrm{P}+p=0 \text{ for the disturbed orbit, } r=\mathfrak{a}+\int\dot{r}_{\mathfrak{a}}dt \end{array}\right\} (22),$$

$\mathfrak{a}$ and a being the radii of the hollow cylindric interior, or free boundary, and of the external fixed boundary, and $\dot{r}_{\mathfrak{a}}$ the value of $\dot{r}$ where r is approximately equal to $\mathfrak{a}$. The condition $\mathrm{T}=c/r$ simplifies (9) and (14) to

$$\mathfrak{g}=-\frac{1}{m}\frac{dw}{dr}, \text{ and } \tau=\frac{iw}{mr} \quad . \quad . \quad (23);$$

$$\frac{d^2w}{dr^2}+\frac{1}{r}\frac{dw}{dr}-\frac{i^2w}{r^2}-m^2w \quad . \quad . \quad (24),$$

and by (7) we have $\varpi=\frac{1}{m}\left(n-\frac{ic}{r^2}\right)w$ . . (25).

Hence $$w=\mathrm{CI}_i(mr)+\mathfrak{C}\mathfrak{I}_i(mr) \quad (26);$$

and the equation of condition for the fixed boundary (radial velocity zero there) gives

$$\mathrm{CI}'_i(ma)+\mathfrak{C}\mathfrak{I}'_i(ma)=0 \quad . \quad (27).$$

To find the other equation of condition we must first find an expression for the disturbance from circular figure of the free inner boundary. Let for a moment r, θ be the co-ordinates of one and the same particle of fluid. We shall have

$$\theta=\int\dot{\theta}dt;\ \text{and } r=\int\dot{r}dt+r_0,$$

where r_0 denotes the radius of the "mean circle" of the particle's path.

Hence to a first approximation,

$$\theta = \frac{ct}{r^2} \quad . \quad . \quad . \quad . \quad (28),$$

and therefore, by (6)

$$\dot{r} = \mathfrak{r} \cos mz \sin \left(n - \frac{ic}{r^2}\right) t\,;$$

whence

$$r = r_0 - \frac{\mathfrak{r}}{n - \frac{ic}{r^2}} \cos mz \cos (nt - i\theta) \quad . \quad . \quad (29),$$

Hence the equation of the free boundary is

$$r = \mathfrak{a} - \frac{\mathfrak{r}_{(r=\mathfrak{a})}}{n - i\omega} \cos mz \cos (nt - i\theta) \quad . \quad (30)$$

where

$$\omega = \frac{c}{\mathfrak{a}^2} \quad . \quad . \quad . \quad . \quad (31).$$

Hence at (r, θ, z) of this surface we have, from $\mathrm{P} = \int \frac{\mathrm{T}^2 dr}{r}$, of (6) above,

$$\mathrm{P} = \frac{\mathrm{T}^2}{r} (r - \mathfrak{a})$$

$$= -\frac{c^2}{\mathfrak{a}^3} \frac{\mathfrak{r}(r=\mathfrak{a})}{n - i\omega} \cos mz \cos (nt - i\theta) \quad . \quad . \quad (32).$$

Hence, and by (6), and (26), and (25), and (23), the condition $\mathrm{P} + p = 0$ at the free boundary gives

$$\frac{c^2}{\mathfrak{a}^3} [\mathrm{C}\mathrm{I}'_i\,(m\mathfrak{a}) + \mathfrak{C}\mathfrak{I}'_i\,(m\mathfrak{a})] + \frac{(n - i\omega)^2}{m} [\mathrm{C}\mathrm{I}_i(m\mathfrak{a}) + \mathfrak{C}\mathfrak{I}_i(m\mathfrak{a})] = 0 \quad (33).$$

Eliminating $\mathrm{C}/\mathfrak{C}$ from this by (27) we get an equation to determine n, by which we find

$$n = \omega(i \pm \sqrt{\mathrm{N}}) \quad . \quad . \quad . \quad (34),$$

where N is an essentially positive numeric.

II.—Sub-case.

A very interesting Sub-case is that of $a = \infty$, which, by (27), makes $\mathrm{C} = 0$; and therefore, by (33), gives

$$\mathrm{N} = m\mathfrak{a} \frac{-\mathfrak{I}'(m\mathfrak{a})}{\mathfrak{I}(m\mathfrak{a})} \quad . \quad (35).$$

Whether in Case II. or Sub-case II. we see that the disturbance consists of an undulation travelling round the cylinder with angular velocity

$$\omega\left(1+\frac{\sqrt{N}}{i}\right), \text{ or } \omega\left(1-\frac{\sqrt{N}}{i}\right)$$

or of two such undulations superimposed on one another, travelling round the cylinder with angular velocities greater than and (algebraically) less than the angular velocity of the mass of the liquid at its free surfaces by equal differences. The propagation of the wave of greater velocity is in the same direction as that in which the liquid revolves; the propagation of the other is in the contrary direction when $N > i^2$ (as it certainly is in some cases).

If the free surface be started in motion with one or other of the two principal angular velocities (34), or linear velocities $\mathfrak{a}\omega\left(1 \pm \frac{\sqrt{N}}{i}\right)$, and the liquid be then left to itself, it will perform the simple harmonic undulatory movement represented by (6), (26), (23). But if the free surface be displaced to the corrugated form (30) and then left free either at rest or with any other distribution of normal velocity than either of those, the corrugation will, as it were, split into two sets of waves travelling with the two different velocities $\mathfrak{a}\omega\left(1 \pm \frac{\sqrt{N}}{i}\right)$.

The case $i=0$ is clearly exceptional, and can present no undulations travelling round the cylinder. It will be considered later.

The case $i=1$ is particularly important and interesting. To evaluate N for it remark that

$$\left.\begin{array}{l} I_1(mr) = I_0'(mr) \\ \text{and} \quad \mathfrak{I}_1(mr) = \mathfrak{I}_0'(mr) \end{array}\right\} \quad . \quad . \quad . \quad (36).$$

Now the general solution of (24) is

$$\left.\begin{array}{l} w = \left(E + D \log \dfrac{1}{mr}\right)\left(1 + \dfrac{m^2r^2}{2^2} + \dfrac{m^4r^4}{2^2.4^2} + \&c.\right) \\ \quad + D\left(\dfrac{m^2r^2}{2^2} S_1 + \dfrac{m^2r^2}{2^2.4^2} S_2 + \&c.\right) \end{array}\right\} \quad . \quad (36),$$

where E and D are constants Hence according to our notation

$$\mathrm{I}_0(mr) = 1 + \frac{m^2r^2}{2^2} + \frac{m^4r^4}{2^2.4^2} + \&c. \quad . \quad . \qquad (37),$$

the constant factor being taken so as to make $\mathrm{I}_0(0) = 1$.

Stokes * investigated the relation between E and D to make $w = 0$ when $r = \infty$ and found it to be

$$\left.\begin{array}{l} \mathrm{E/D} = \log 8 + \pi^{-\frac{1}{2}}\Gamma'\tfrac{1}{2} = +2{\cdot}079442 - 1{\cdot}963510 = {\cdot}11593 \\ \text{or, to 20 places,} \quad \mathrm{E/D} = {\cdot}11593\ 15156\ 58412\ 44881 \end{array}\right\} (38).$$

Hence, and by convenient assumption for constant factor,

$$\left.\begin{array}{l} \mathfrak{I}_0(mr) = \log \dfrac{1}{mr}\left(1 + \dfrac{m^2r^2}{2^2} + \dfrac{m^4r^4}{2^2.4^2} + \&c.\right) \\ \qquad + \dfrac{m^2r^2}{2^2}(\mathrm{S}_1 + {\cdot}11593) + \dfrac{m^4r^4}{2^2.4^2}(\mathrm{S}_2 + {\cdot}11593) + \&c. \end{array}\right\} (39).$$

It is to be remarked that the series in (36) and (39) are convergent however great be mr; though for values of mr exceeding 6 or 7 the semi-convergent expressions † will give the values of the functions nearly enough for most practical purposes, with much less arithmetical labour.

From (37) and (39) we find by differentiation

$$\left.\begin{array}{l} \mathrm{I}_1(mr) = \dfrac{mr}{2} + \dfrac{m^3r^3}{2^2.4} + \dfrac{m^5r^5}{2^2.4^2.6} + \&c. \\ \mathrm{I}_1'(mr) = \dfrac{1}{2} + \dfrac{3m^2r^2}{2^2.4} + \dfrac{5m^4r^4}{2^2.4^2.6} + \&c. \end{array}\right\} \quad . \quad . \quad . \qquad (40).$$

* "On the Effect of Internal Friction on the Motion of Pendulums," equations (93) and (106).—*Cambridge Phil. Trans., Dec.* 1850.

P.S.—I am informed by Mr J. W. L. Glaisher that Gauss, in section 32 of his "Disqusitiones Generales circa seriem infinitam $1 + \frac{\alpha.\beta}{1.\gamma}x$ + &c.," (Opera, vol. iii. p. 155), gives the value of $-\pi^{-\frac{1}{2}}\Gamma'\frac{1}{2}$, or $-\psi(-\frac{1}{2})$ in his notation, to 23 places as follows :—

1·96351 00260 21423 47944 099.

Thus it appears that the last figure in Stokes' result (106) ought, as in the text, to be 0 instead of 2. In Callet's Tables we find

$$\log_\epsilon 8 = 2{\cdot}07944\ 15416\ 79835\ 92825,$$

and subtracting the former number from this we have the value of E to 20 places given in the text.

† Stokes, *ibid.*

$$\mathfrak{K}_1(mr)=\frac{1}{mr}-\frac{mr}{2^2}[-1+2(\mathrm{S}_1+\cdot1159315)]+\frac{m^3r^3}{2^2.4^2}[-1+2(\mathrm{S}_2+\cdot1159315)]+\&c.$$

$$-\log\frac{1}{mr}\left(\frac{mr}{2}+\frac{m^3r^3}{2^2.4}+\frac{m^5r^5}{2^2.4^2.6}+\&c.\right)$$

$$\mathfrak{K}_1'(mr)=\frac{-1}{m^2r^2}-\frac{1}{2^2}[-3+2(\mathrm{S}_1+\cdot1159315)]+\frac{m^2r^2}{2^2.4^2}[7-6(\mathrm{S}_2-\cdot1159315)]+\&c.$$

$$-\log\frac{1}{mr}\left(\frac{1}{2}+\frac{3m^2r^2}{2^2.4}+\frac{5m^4r^4}{2^2.4^2.6}+\&c.\right)$$

For an illustration of Case II. with $i=1$, suppose $m\mathfrak{a}$ to be very small. Remarking that $\mathrm{S}_1=1$, we have

$$\mathrm{N}=\frac{-m\mathfrak{a}\mathfrak{K}_1'(m\mathfrak{a})}{\mathfrak{K}_1(m\mathfrak{a})}=\frac{1+\dfrac{m^2\mathfrak{a}^2}{2}\left[\log\dfrac{1}{m\mathfrak{a}}-\frac{1}{2}+\cdot1159\right]}{1-\dfrac{m^2\mathfrak{a}^2}{2}\left[\log\dfrac{1}{m\mathfrak{a}}+\frac{1}{2}+\cdot1159\right]}$$

$$=1+m^2\mathfrak{a}^2\left(\log\frac{1}{m\mathfrak{a}}+\cdot1159\right)\quad . \quad (42);$$

Hence in this case at all events $\mathrm{N}>i^2$; and the angular velocity of the slow wave, in the reverse direction to that of the liquid's revolution, is

$$-n=\tfrac{1}{2}\omega m^2\mathfrak{a}^2\left(\log\frac{1}{m\mathfrak{a}}+\cdot1159\right)\quad . \quad . \quad (43).$$

This is very small in comparison with

$$2\omega+\tfrac{1}{2}\omega m^2\mathfrak{a}^2\left(\log\frac{1}{m\mathfrak{a}}+\cdot1159\right)\quad . \quad . \quad (44),$$

the angular velocity of the direct wave; and therefore clearly if the initial normal velocity of the surface when left free after being displaced from its cylindrical figure of equilibrium be zero or anything small, the amplitude of the quicker direct wave will be very small in proportion to that of the reverse slow one.

Case III.

A slightly disturbed vortex column in liquid extending through all space between two parallel planes; the undisturbed column consisting of a core of uniform vorticity (that is to say, rotating like a solid) surrounded by irrotationally revolving liquid with no slip at

the cylindric interface. Denoting by a the radius of this cylinder we have

$$\left.\begin{aligned} &\mathrm{T}=\omega r\ , \text{ where } r<a \\ \text{and}\quad &\mathrm{T}=\omega\frac{a^2}{r}\quad ,, \quad r>a \end{aligned}\right\} \quad . \quad . \quad (45).$$

Hence (13), (14) hold for $r<a$, and (23), (24) for $r>a$.

Going back to the form of assumption (6) we see that it suits the condition of rigid boundary planes if Oz be perpendicular to them, O in one of them, and the distance between them π/m.

The conditions to be fulfilled at the interface between core and surrounding liquid are that ϱ and w must have the same values on the two sides of it: it is easily proved that this implies also equal values of ϖ on the two sides. The equality of ϱ on the two sides of the interface gives, by (13) and (23),

$$\left\{\frac{\left(i\omega-n\right)\left[\left(i\omega-n\right)\frac{dw}{dr}+\frac{2i\omega}{r}w\right]}{4\omega^2-(i\omega-n)^2}\right\}^{\text{internal}}_{r=a}=-\left(\frac{dw}{dr}\right)^{\text{external}}_{r=a} \quad . \quad (46):$$

and from this and the equality of w on the two sides we have

$$\frac{\left(i\omega-n\right)\left[\left(i\omega-n\right)\left(\frac{dw}{wdr}\right)^{\text{internal}}_{r=a}+\frac{2i\omega}{a}\right]}{4\omega^2-(i\omega-n)^2}=-\left(\frac{dw}{wdr}\right)^{\text{external}}_{r=a} \quad . \quad (47).$$

The condition that the liquid extends to infinity all round makes $w=0$ when $r=\infty$. Hence the proper integral of (24) is of the form $\mathfrak{I}_i$: and the condition of undisturbed continuity through the axis shows that the proper integral of (13) is of the form J_i. Hence

$$\left.\begin{aligned} &w=\mathrm{C}\mathrm{J}_i\,(\nu r) \text{ for } r<a \\ \text{and}\quad &w=\mathfrak{C}\mathfrak{I}_i\,(mr)\ ,, \quad r>a \end{aligned}\right\} \quad . \quad . \quad (48);$$

by which (47) becomes

$$\frac{(i\omega-n)\left[(i\omega-n)\frac{\nu\mathrm{J}_i'(\nu a)}{\mathrm{J}_i(\nu a)}+\frac{2i\omega}{a}\right]}{4\omega^2-(i\omega-n)^2}=\frac{-m\mathfrak{I}_i'(ma)}{\mathfrak{I}_i(ma)} \quad (49);$$

or by (15),

$$\frac{\mathrm{J}_i'(q)}{q\mathrm{J}_i(q)}+\frac{i}{q^2\lambda}=\frac{-\mathfrak{I}_i'(ma)}{ma\mathfrak{I}_i(ma)} \quad . \quad . \quad (50),$$

where $$\lambda = \frac{i\omega - n}{2\omega} \quad . \quad . \quad . \quad . \quad (51),$$

and $$q^2 = m^2a^2\frac{1-\lambda^2}{\lambda^2} \quad . \quad . \quad . \quad (52).$$

Remarking that $J_i(q)$ is the same for positive and negative values of q, and that it passes from positive through zero to a finite negative maximum, thence through zero to a finite positive maximum and so on an infinite number of times, while q is increased from 0 to ∞, we see that while λ is increased from -1 to 0 the first member of (50) passes an infinite number of times continuously through all real values from $-\infty$ to $+\infty$: and that it does the same when λ is diminished from $+1$ to 0. Hence (50), regarded as a transcendental equation in λ, has an infinite number of roots between -1 and 0 and an infinite number between 0 and $+1$. And it has no roots except between -1 and $+1$, because its second member is clearly positive, whatever be ma; and its first member is essentially real and negative for all real values of λ except between -1 and $+1$, as we see by remarking that when $\lambda^2 > 1, -q^2$ is real and positive, and $-J'_i(q)/qJ_i(q)$ is real and $> i/(-q^2)$, while $i/q^2\lambda$, whether positive or negative, is of less absolute value than $i/(-q^2)$.

Each of the infinite number of values of λ yielded by (50) gives, by (51) and (13), a solution of the problem of finding simple harmonic vibrations of a columnar vortex, with m of any assumed value. All possible simple harmonic vibrations are thus found: and summation after the manner of Fourier for different values of m, with different amplitudes and epochs and different epochs, gives every possible motion, deviating infinitely little from the undisturbed motion in circular orbits.

The simplest Sub-case, that of $i = 0$, is curiously interesting. For it (50), (51), (52) give

$$\frac{J_0'(q)}{qJ_0(q)} = \frac{-\mathfrak{I}_0'(ma)}{ma\mathfrak{I}_0(ma)} \quad . \quad . \quad . \quad (53),$$

and $$n = \frac{2\omega ma}{\sqrt{(m^2a^2 + q^2)}} \quad . \quad . \quad . \quad (54).$$

The successive roots of (53), regarded as a transcendental equation in q, lie between the 1st, 3d, 5th - - - roots of $J_0(q) = 0$, in order of ascending values of q, and the next greater roots of $J'_0(q) = 0$,

coming nearer and nearer down to the roots of J_0, the greater they are. They are easily calculated by aid of Hansen's Tables of Bessel's functions J_0 and J_1 (which is equal to J'_0) from $q=0$ to $q=20$.* When ma is a small fraction of unity, the second member of (53) is a large number, and even the smallest root exceeds by but a small fraction the first root of $J_0(q)=0$, which, according to Hansen's Table, is 2·4049, or approximately enough for the present 2·4. In every case in which q is very large in comparison with ma, whether ma is small or not, (54) gives

$$n=\frac{2\omega ma}{q} \text{ approximately.}$$

Now going back to (6) we see that the summation of two solutions to constitute waves propagated along the length of the column, gives

$$\left.\begin{array}{ll}\dot{r}=-\varrho\sin(nt-mz)\,; & r\dot{\theta}=\mathrm{T}+\tau\cos(nt-mz)\\ \dot{z}=w\cos(nt-mz)\,; & p=\mathrm{P}+\varpi\cos(nt-mz)\end{array}\right\}\quad(55).$$

The velocity of propagation of these waves is n/m. Hence when q is large in comparison with ma, the velocity of longitudinal waves is $2\omega a/q$, or $2/q$ of the translational velocity of the surface of the core in its circular orbit. This is 1/1·2, or $\frac{5}{6}$ of the translational velocity, in the case of ma small, and the *mode* corresponding to the smallest root of (53). A full examination of the internal motion of the core, as expressed by (55), (13), (48), (15) is most interesting and instructive. It must form a more developed communication to the Royal Society.

The Sub-case of $i=1$, and ma very small, is particularly interesting and important. In it we have, by (42), for the second member of (50), approximately,

$$\frac{-\mathfrak{I}_1'(ma)}{ma\mathfrak{I}_1(ma)}=\frac{1}{m^2a^2}\left[1+m^2a^2\left(\log\frac{1}{ma}+\cdot1159\right]\right).\quad(56).$$

In this case the smallest root, q, is comparable with ma, and all the others are large in comparison with ma. To find the smallest, remark that, when q is very small, we have to a second approximation,

$$\frac{J_1'(q)}{qJ_1(q)}=\frac{1}{q^2}-\frac{1}{4}\quad . \quad . \quad . \quad (57).$$

* Republished in Lömmel's "Besselsche Functionen," Leipzig, 1868.

Hence (50), with $i=1$, becomes, to a first approximation,

$$\frac{1}{q^2}\left(1+\frac{1}{\lambda}\right)=\frac{1}{m^2a^2} \quad . \quad . \quad . \quad (58).$$

This and (52) used to find the two unknowns λ and q^2, give

$$\lambda=\tfrac{1}{2}, \text{ and } q^2=3m^2a^2,$$

for a first approximation. Now, with $i=1$, (51) becomes

$$\lambda=\tfrac{1}{2}\left(1-\frac{n}{\omega}\right),$$

and therefore n/ω is infinitely small. Hence (52) gives for a second approximation

$$q^2=3m^2a^2\left(1+\frac{8n}{3\omega}\right) \quad . \quad . \quad . \quad (59);$$

and we have

$$\frac{1}{q^2\lambda}=\frac{2}{3}\frac{1}{m^2a^2}\left(1-\frac{5n}{3\omega}\right) \quad . \quad . \quad . \quad (60).$$

Using now (57), (59), (60), and (56) in (50), we find to a second approximation

$$\frac{1}{3ma^2}\left(1-\frac{8n}{3\omega}\right)-\frac{1}{4}+\frac{2}{3ma^2}\left(1-\frac{5n}{3\omega}\right)=\frac{1}{m^2a^2}\left[1+m^2a^2\left(\log\frac{1}{ma}+{\cdot}1159\right)\right];$$

whence

$$\frac{-n}{\omega}=\frac{1}{2}m^2a^2\left(\log\frac{1}{ma}+\frac{1}{4}+{\cdot}1159\right) \quad . \quad . \quad (61).$$

Compare this result with (43) above. The fact that, as in (43), $-n$ is positive in (61), shows that in this case also the direction in which the disturbance travels round the cylinder is *retrograde* (or opposite to that of the translation of fluid in the undisturbed vortex); and, as is to be expected, the values of $-n$ are approximately equal in the two cases, when ma is small enough; but it is smaller by a relatively small difference in (60) than in (43), as is also to be expected.

The case of ma small and $i>1$ has a particularly simple approximate solution for the smallest q-root of the transcendental (50). With any value of i instead of unity we still have (58), as a first approximation for q small. Eliminating q^2/m^2a^2 between this and (52) we still find $\lambda=\frac{1}{2}$; but instead of $n=0$ by (51), we now have $n=(i-1)\omega$. Thus is proved the solution for waves of deformation of sectional figure travelling round a cylindrical vortex, announced thirteen years ago without proof in my first article respecting Vortex Motion.*

* "Vortex Atoms," Proc. Roy. Soc. Edin., Feb. 18, 1867.

4. The Structure of the Comb-like Branchial Appendages and the Teeth of the Basking Shark (*Selache maxima*). By Professor Turner, M.B., F.R.S.

Attention was drawn to the statements made on the position of peculiar comb-like fringes on the branchiæ of the basking shark by Gunnerus, Pennant, Low, Mitchell, Foulis, Brito Capello, Cornish, Steenstrup, Pavesi, P. & H. Gervais, Percival Wright, and Allman, and to the structure of these fringes by Hannover and MM. Gervais. The author then proceeded to give a detailed description of the structure of the plates forming these fringes from a specimen presented to him by the Rev. M. Harvey of St. John's, Newfoundland, the general summary of which is as follows: the whole periphery of a plate consisted of a hard unvascular dentine, the tubes in which were very distinctive; in a considerable part of the shaft these tubes arose from a single central pulp cavity, but in the semilunar attached base of the plate the single central cavity did not exist, but was replaced by a set of anastomosing vascular canals, which collectively represented a pulp cavity, and which gave origin to numerous characteristic dentine tubes. It was suggested that these plates were developed in the mucous membrane covering the branchiæ after the manner of teeth. Although these plates act, like whalebone plates, to separate from the water the small organisms on which this shark lives, they were shown to be essentially different in structure and mode of origin, the matrix of whalebone being a cornification of the ephithelium of the palate derived from the epiblast, whilst the matrix in the shark's branchial plates is a calcification of dermal or sub-epithelial structure, and therefore derived from the mesoblast. Reference was made to the observations of Andrew Smith on *Rhinodon*, in which an apparatus having a similar office, but probably a different structure, was seen in that shark; and to the observations of Van Beneden on a comb-like fringe found fossilised in the Antwerp Crag.

The structure of the small conical teeth of the basking shark was then described from a specimen also presented by the Rev. M. Harvey. They were shown to have an external layer of hard unvascular dentine, covering an extensive core in which relatively large

vascular canals anastomosed to form a network. From these canals numerous dentine tubes arose. Both the core of the tooth and the anastomosing canals with their dentine tubes in the semi-lunar base of a comb-plate were examples of vaso-dentine. These plates in the basking shark may be regarded as an example of excessively developed branchial teeth, a development which is co-related with the small size and simple form of the maxillary and mandibular teeth, with the non-predaceous habits of the fish, and with the particular nature of the food on which it lives.

This communication will be printed *in extenso* in the "Journal of Anatomy and Physiology," April 1880.

5. Preliminary Report on the Tunicata of the "Challenger" Expedition. By W. A. Herdman, B.Sc., Baxter Scholar in Natural Science in the University of Edinburgh.

(*By permission of the Lords Commissioners of the Treasury.*)

I. Ascidiadæ.

Last year the Tunicata collected during the "Challenger" Expedition were entrusted to me for description by Sir Wyville Thomson.

The entire collection comprises from 150 to 200 species, the majority of which are new to science.

As yet only some of the more abnormal *Synascidiæ* and about half the *Ascidiæ simplices* have been carefully examined. The present paper is the preliminary report on the Ascidiadæ, the first family of the *Ascidiæ simplices.*

The family Ascidiadæ is synonymous with Savigny's genus *Phallusia*, or Forbes' *Ascidia,* and includes those simple ascidians which are, as a rule, externally characterised by an eight-lobed branchial and a six-lobed atrial aperture, as distinguished from two other families—the Cynthiadæ, with both apertures four-lobed, and the Molgulidæ, having the branchial six- and the atrial four-lobed.

This point, the number of lobes round the apertures, though a most important diagnostic, does not hold good for all Ascidiadæ without exception. Indeed, any one of the characters of the three families, if employed singly, will be found, while sufficing in the

majority of cases, to break down in regard to a few species. For example, *Ascidia involuta* has the entire body encrusted with sand grains and shells, a condition characteristic of the MOLGULIDÆ; simple unbranched tentacles, an important character in the ASCIDIADÆ are also found in *Styela* (CYNTHIADÆ); lastly, the papillated branchial sac of the ASCIDIADÆ can no longer be considered an essential, *Abyssascidia* n. gen., having no papillæ on its longitudinal bars.

The more important characteristics of the ASCIDIADÆ are the following:—

Body sessile, attached.

Branchial aperture eight-lobed, *atrial* six-lobed.

Test gelatinous or cartilaginous.

Branchial sac not conspicuously folded; papillated.

Tentacles unbranched, filiform.

The family includes five known genera, two of which—*Rhopalæa* and *Rhodosoma*—are not represented in the "Challenger" collection, which however includes a new genus—*Abyssascidia*, and a new sub-genus of *Ascidia*—*Pachychlæna*.

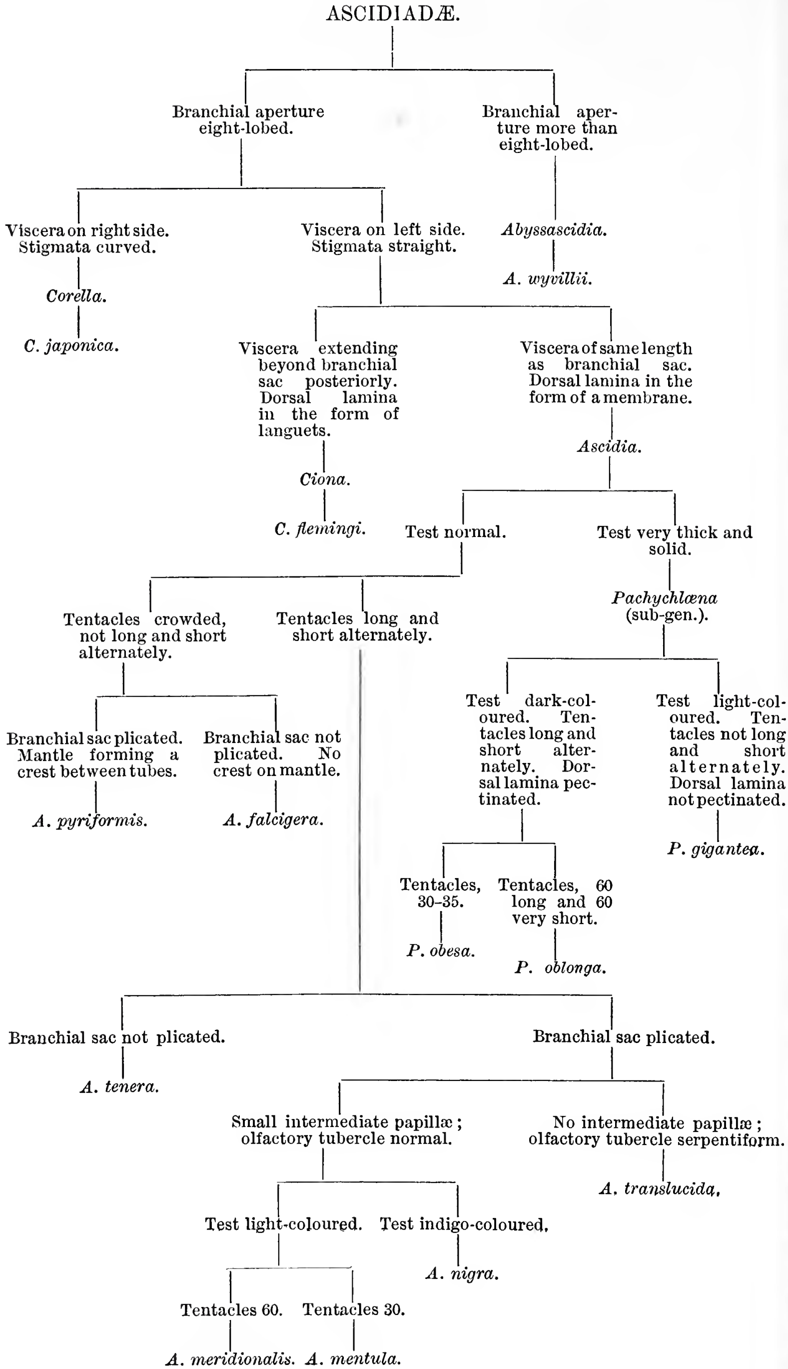
The "Challenger" ASCIDIADÆ are divided into the following genera:—

(1.) *Ciona*, Fleming, 1 species.
(2.) *Ascidia.*, Linn., 8 species.
Pachychlæna, n. sub-gen., 3 species.
(3.) *Abyssascidia*, n. gen., 1 species.
(4.) *Corella*, Hancock, 1 species.

Of these fourteen species, twelve are new to science. The majority of the specimens are from shallow water (10 to 100 fathoms); two (*Ascidia tenera* and *Ascidia meridionalis*) are from moderate depths (245 and 600 fathoms); while one (*Abyssascidia wyvillii*) was obtained at the great depth of 2600 fathoms.

The following table * shows the different genera and species synoptically, and gives a few of their more important distinctive characters.

* On account of the meagre, and in some cases insufficient, manner in which ascidians have often been described, it has been found impossible to extend the table so as to include the species already known.


ASCIDIADÆ.
Branchial aperture eight-lobed.
Branchial aperture more than eight-lobed.
Viscera on right side. Stigmata curved.
Viscera on left side. Stigmata straight.
Abyssascidia.
A. wyvillii.
Corella.
C. japonica.
Viscera extending beyond branchial sac posteriorly. Dorsal lamina in the form of languets.
Viscera of same length as branchial sac. Dorsal lamina in the form of a membrane.
Ciona.
C. flemingi.
Ascidia.
Test normal.
Test very thick and solid.
Pachychlœna (sub-gen.).
Tentacles crowded, not long and short alternately.
Tentacles long and short alternately.
Test dark-coloured. Tentacles long and short alternately. Dorsal lamina pectinated.
Test light-coloured. Tentacles not long and short alternately. Dorsal lamina not pectinated.
Branchial sac plicated. Mantle forming a crest between tubes.
Branchial sac not plicated. No crest on mantle.
A. pyriformis.
A. falcigera.
P. gigantea.
Tentacles, 30–35.
Tentacles, 60 long and 60 very short.
P. obesa.
P. oblonga.
Branchial sac not plicated.
Branchial sac plicated.
A. tenera.
Small intermediate papillæ; olfactory tubercle normal.
No intermediate papillæ; olfactory tubercle serpentiform.
A. translucida.
Test light-coloured.
Test indigo-coloured.
A. nigra.
Tentacles 60.
Tentacles 30.
A. meridionalis.
A. mentula.

Ciona flemingi, n. sp.

External appearance.—Shape somewhat pyriform, elongated; anterior end wide, posterior much narrower, forming a short stalk turned ventrally and attached to a fragment of nullipore by the extremity of its right side. Apertures at the anterior end, inconspicuous; the branchial near the ventral edge, the atrial near the dorsal edge. They are equally far forward, the most anterior point being placed between them. Surface smooth. Colour light-grey. Length, 2·2 cm.; breadth, 8 mm.

Test thin, soft, almost gelatinous, transparent; vessels few.

Mantle normal; musculature rather feebly developed, consisting chiefly of a few straight bundles running longitudinally.

Branchial sac rather thick, small and shrunken looking; internal longitudinal vessels coarse and strong, much crumpled, bearing knob-like papillæ at their intersections with the transverse vessels; no intermediate papillæ; stigmata elongate-elliptical, two or three in a mesh.

Dorsal lamina reduced to a series of short tusk-like languets.

Tentacles simple, all one length, twelve in number.

Olfactory tubercle heart-shaped.

Viscera extending beyond the branchial sac posteriorly.

A single specimen labelled "Off Gomera, 75 fms."

(*Pachychlœna*, n. sub-gen. of *Ascidia.*)

Pachychlœna oblonga, n. sp.

External appearance.—Shape irregularly oblong, widest about the middle, narrowing somewhat towards the anterior end, which is obtuse and flattened; posterior end rather drawn out, attached to the interior of a large *Cardium*, which is in a three-quarters closed condition, constricting the test of the ascidian. Branchial aperture not terminal, placed on the right side near the ventral edge and about one-fifth of the distance to the mouth of the shell; it is directed ventrally posteriorly and to the right. Atrial aperture on the right side, near the dorsal margin and slightly anterior to the branchial aperture; it is directed dorsally and anteriorly. Seen from the ventral aspect it seems as if the anterior end had been bent over towards the right side, thus accounting for the lateral position of the branchial aperture.

Surface smooth but mamillated, very strongly on the anterior half, and especially near the branchial aperture where a few more sharply cut papillæ are visible. Colour light smoky brown, rather deeper in tint at the anterior end. Length, 8 cm; breadth, 4 cm.

Test cartilaginous, thickish, of a light greyish-brown colour throughout. Vascular trunks enter the test on the left side about half-way down, and large vessels ramify on the inner surface.

Mantle moderately muscular.

Branchial sac plicated longitudinally; the transverse vessels divide the grooves into rows of pouches, which are rather irregularly placed, and have no relation to the internal longitudinal bars. Transverse vessels all nearly of one size; meshes transversely oblong, containing each eight to ten stigmata; papillæ large and irregularly shaped, no intermediate smaller ones.

Dorsal lamina ribbed transversely, and strongly pectinated at the margin, a rib running out to the apex of each tooth.

Tentacles numerous, filiform, sixty-two large ones and about the same number of very minute intermediate ones. These last are so small as to be easily overlooked; usually one is placed between each pair of large tentacles, but in some spaces there appears to be none.

Olfactory tubercle large, irregularly oval in outline.

One specimen, in excellent condition, from Station 162 (Bass' Straits), 38 to 40 fathoms.

This species agrees in the thickness of its test with the next two species, and in the structure of the branchial sac with these and some others. The difference of size in the tentacles, the condition of the dorsal lamina, the transversely oblong meshes, and the absence of intermediate papillæ are all important characters.

Pachychlæna obesa, n. sp.

External appearance.—Shape unknown, on account of the absence of the greater part of the test, probably oval or irregularly spherical. Apertures not distant, depressed. Surface smooth, mamillated. Colour dark earthy-brown. Length probably about 10 cm.; breadth about 6 cm.

Test cartilaginous, thick (8 mm.), solid, opaque; vessels visible on the internal surface.

Mantle thick on the right (branchial) side of the body, and on the tubes, but not very muscular; membranous on the large distended left (visceral) side. Tubes long and rather narrow.

Branchial sac long and narrow, pointed at the posterior end; plicated longitudinally, the internal longitudinal bars being placed on the ridges. Meshes transversely oblong, each containing about six stigmata. Papillæ all of one size, irregular in shape, often cleft or lobed.

Dorsal lamina ribbed transversely, and bearing small teeth on its free margin.

Tentacles filiform, slender, alternately large and small; probably 30 to 35 in number.

Two specimens from Station 162 (Bass' Straits), 38 to 40 fathoms.

In both specimens almost the whole of the test is gone, which makes it impossible to give the external characters with certainty. The branchial aperture also is damaged in both to such an extent that the exact number of tentacles and the nature of the olfactory tubercle cannot be determined. This species is closely allied to the last.

Pachychlæna gigantea, n. sp.

External appearance.—Shape, as far as can be made out, irregularly oblong, the right side being larger than the left. Probably attached by the posterior part of the ventral edge. Branchial aperture terminal, on a large irregularly rounded projection turned towards the left side. Atrial aperture on the dorsal edge, also on a large projection, situated more than one-third of the way down. Lobes of apertures irregular but prominent. Surface very irregular and almost covered by *Polyzoa*, *Hydroida*, *Algæ*, &c. Colour of a warm yellowish-grey where the test itself is seen. Length about 12 cm.; breadth, 5 to 7 cm.

Test cartilaginous, very thick (2 mm. to 4 cm.) and solid, white in mass with a hyaline tint where thin, yellowish-grey on the external surface. Large vessels ramify in the inner layer; the vascular trunks probably enter the test at the base of the right side towards the ventral edge.

Mantle strongly muscular over the right side and on the tubes,

membranous on the left side which is large and projecting. Tubes very long and diverging at more than a right angle.

Branchial sac very thick, coarse, and opaque, of a brown colour. Plicated longitudinally, and the grooves divided into pouches as in the last two species. On the external aspect the wide transverse vessels are connected by equally wide irregularly placed longitudinal vessels, thus forming a network of quadrangular meshes, each of which contains about four rows of stigmata. Meshes on the internal surface much elongated transversely, each containing 15 to 20 stigmata. Papillæ at the corners, no smaller intermediate ones.

Dorsal lamina wide, strongly ribbed transversely, but not pectinated.

Tentacles long and stout, about 60 in number, large and small not alternating.

Olfactory tubercle heart-shaped, $3\frac{1}{2}$ mm. long.

Two specimens from Simon's Bay, 10 to 20 fathoms.

In both, half the test has been cut away; the ventral edge, the posterior end, and part of each side is wanting.

This species and the two preceding are allied forms. They agree in the great thickness and solidity of the test, in the transversely elongated meshes of the branchial sac, and in the absence of small intermediate papillæ. They also possess that minute longitudinal plication of the stigmatic part of the branchial sac which is found in two other new species (*Ascidia pyriformis* and *Ascidia translucida*), and on account of which Verrill proposed to separate *Ascidia complanata* under the generic title of *Ascidiopsis.* This structure, however, is also to be seen in *Ascidia mentula*, *Ascidia sordida*, and several other species, some of which differ from each other in important points. On account of this, I think it unadvisable to use the plication of the branchial sac as a characteristic in breaking up *Ascidia.* If any division of the genus is necessary, the three species just described form a very natural section characterised by the several points of resemblance mentioned above, and worthy of being separated, not on account of the similarity in structure of their branchial sacs, but because of the remarkable thickness and solidity of their tests suggesting *Pachychlæna* (παχύς and χλαῖνα) as an appropriate subgeneric name.

Ascidia meridionalis, n. sp.

External appearance.—Shape somewhat variable, generally oval, the anterior end being slightly narrower than the posterior, flattened laterally, base rounded; attached by posterior end and part of left side. Branchial aperture terminal, placed on a large conical papilla of which the apex is inclined ventrally and to the right. Atrial aperture to the right of or on the dorsal edge, and about one-third of the way down, slightly projecting. Surface slightly velvety, with minute processes scattered over it. Colour light brown or horn-coloured. Length about 12 cm.; breadth about 8 cm.

Test softish, tears easily, from 1·5 to 6 mm. thick, the left side being thicker than the right. Vascular trunks enter about the middle of the left side near the ventral margin, large vessels visible on the inner surface, which is smooth and shining.

Mantle moderately muscular.

Branchial sac minutely undulated longitudinally. Three small transverse vessels between each pair of large ones. Papillæ, and generally smaller intermediate ones present.

Dorsal lamina broad, ribbed transversely.

Tentacles simple, filiform, about 60 in number, placed long and short alternately.

Olfactory tubercle semilunar, horns pointing anteriorly.

Several specimens from Station 320 (off the coast of Buenos Ayres), 600 fathoms, and two specimens from Station 313 (Strait of Magellan), 55 fathoms.

Ascidia mentula, O. F. Müller.

This species was obtained at four localities at Kerguelen Island, in depths of from 10 to 60 fathoms.

Ascidia vasculosa, n. sp.

External appearance.—Shape very irregular, somewhat quadrangular, depressed; anterior end a little prolonged and narrowed. Attached by the left side near the base. Branchial aperture not quite terminal, being on the right side of the anterior extremity. Atrial aperture also on the right side, nearer the dorsal than the

ventral edge, and a little in front of the middle. Both apertures rather depressed and concealed. Surface very irregular, grooved and mamillated; *Synascidiæ*, annelide-tubes, &c., adhering to it. Colour light yellowish-grey, not opaque, rather hyaline at the edges, and showing everywhere numerous blood-vessels ramifying near the surface. The terminal twigs of the vessels with their swollen ends are a prominent feature. Length, 9 cm.; breadth, 5·6 cm.

Test solid looking, varies in thickness from less than ·5 mm. on the right side behind the middle to 1·5 cm. on the left side near the place of attachment. Apertures lobed indistinctly; vascular trunks enter on the left side near the ventral edge and branch usually dichotomously, the terminal twigs ending in swollen knobs. The test shows no bladder cells. It contains the small spherical fusiform and stellate cells, and many minute granules. Crystals or concretions are also present, generally in the form of short rods and crosses.

One specimen, the test only, from Royal Sound, Kerguelen Island, 28 fathoms.

It may be considered a somewhat doubtful proceeding to describe an ascidian from the test alone, and certainly in most cases it would not be proper. Still this specimen possessed such well-marked characteristics that I was tempted to give it a name. I believe that the test is distinct from that of all known species, and that when other specimens are found they will be easily recognised. It differs from *Ascidia arachnoidea* in the general shape and the position of the apertures.

Ascidia nigra, Savigny.

One specimen labelled, "Bermuda, shallow water." Length, 6 cm.; breadth, 4 cm.

Ascidia translucida, n. sp.

External appearance.—Shape ellipsoidal, oblong-ovate to oblong; both ends rounded. Attached slightly by left side near base. Apertures sessile, placed on the right side. Branchial nearly terminal and median. Atrial more than a third of the way down, midway between the centre and the dorsal edge. Surface smooth and glossy. Colour very light grey, almost transparent, marked on the left side and the margins by white vascular ramifications. Length, 2·2 cm.; breadth, 1·2 cm.

Test moderately thick and solid, transparent. Vascular trunks enter near the centre of the left side, are of large size and branch freely; none visible on the centre of the right side.

Mantle thin.

Branchial sac longitudinally plicated, showing externally a division into pouches. Meshes nearly square, the transverse extent being slightly the longer; each contains six to eight stigmata. In the centre of each mesh a vertical oblique vessel connects the inter-papillar membrane at the top of the mesh with the transverse vessel at the bottom. Papillæ at the corners long and conical, no intermediate ones.

Dorsal lamina ribbed transversely, edge plain.

Tentacles simple, 30 to 35, long and short alternately.

Olfactory tubercle greatly elongated laterally, disposed in a series of irregular folds.

Three specimens from Kerguelen Island, 28 fathoms. In one specimen the vascular ramifications in the test are more conspicuous than in the other two.

Ascidia tenera, n. sp.

External appearance.—Shape oblong, flattened laterally; posterior end rounded, anterior end rather blunt; attached by the posterior third of the left side. Branchial aperture terminal, directed somewhat ventrally, sessile, lobes well marked. Atrial aperture placed to the right of the dorsal border, about one-third of the way down, sessile, lobes well marked. Surface soft and somewhat velvety, marked with slight creases, mostly longitudinal; near the apertures, especially the branchial, raised into minute pointed projections. Colour light brownish grey or pale horn-colour. Length, 5 cm.; breadth, 3 cm.

Test thin, soft, easily torn, transparent. Vessels moderately developed, trunks enter on the left side near the base.

Mantle very thin, muscular bands delicate, course of alimentary canal visible from both sides.

Branchial sac not plicated. Generally five or seven smaller transverse vessels between a pair of larger ones. Longitudinal internal bars narrow but well marked, bearing papillæ at the

corners of the meshes, and smaller more conical intermediate ones. Stigmata elongated, three or four in each mesh.

Dorsal lamina rather broad, delicately ribbed transversely; edge pectinated, having a small intermediate tooth between each pair of larger ones.

Tentacles filiform, 40 in number, long and short alternately.

Three specimens: one in good condition from Station 311 (West end of Strait of Magellan), 245 fathoms; and two, one of them damaged, from Station 320 (off the coast of Buenos Ayres), 600 fathoms. The two latter are slightly smaller than the dimensions given above.

This species somewhat resembles *Ascidia virginea*, but is undoubtedly distinct from it. That species differs from the present one chiefly in its greater length, its (slightly) greater number of tentacles, the absence of intermediate papillæ, and in the condition of the dorsal lamina—all good characters.

Ascidia pyriformis, n. sp.

External appearance.—Irregularly pear-shaped, anterior end narrow, posterior broad and rounded. Attached by a small area near the posterior end of the left side. Branchial aperture terminal, placed on a long somewhat conical projection turned dorsally. Sides of this projection channelled by eight grooves leading down from between the lobes of the aperture. A strong elevated ridge extending from the base of the projection along the anterior part of the dorsal edge. Atrial aperture sessile, placed at the posterior extremity of this ridge, being more than half-way from the anterior to the posterior end. Surface irregular, prolonged into a few thickish processes for attachment at the base, slightly rough, the globular posterior end encrusted with sand and shell fragments. Colour dull dirty grey. Length, 5 cm.; breadth, 3 cm.

Test remarkably thin, except on the tubes and the ridge connecting them, the latter being very thick.

Mantle moderately muscular over the branchial sac and on the tubes, membranous elsewhere. Tubes long, the branchial measuring 1·2 cm. and the atrial 8 cm. The former has a sharp bend dorsally and to the right above its middle, and at this point a

muscular process about 4 mm. long projects from the dorsal edge. Atrial tube more than half-way down the dorsal edge, and nearly at the posterior end of a crested ridge extending backwards from the branchial tube. The projecting points of the ridge are attached to the inner surface of the test. Right (branchial) side of the body long and narrow, left (visceral) very large, occupying all the ventral part of the body and even appearing on the right side below the branchial sac.

Branchial sac of moderate size, longish, pointed at the dorsal edge of the lower end; longitudinally plicated. The internal longitudinal bars placed on the ridges. Meshes square, with stout papillæ at their corners.

Dorsal lamina ribbed transversely, margin bluntly serrated.

Tentacles very numerous, crowded, long and slender, varying in thickness, but all of much the same length.

One specimen from Port Jackson, 6 fathoms.

Ascidia falcigera, n. sp.

External appearance.—Shape elliptical or nearly round, usually depressed. Area of attachment large, extending from the posterior end half-way up the left side; often expanded at the edge of the base into a thin spreading margin in which small stones are imbedded. Apertures on the upper (right) side, near the anterior end, not far apart. Branchial at or close to the ventral border, atrial near the centre—the latter is the more prominent though neither projects much; lobes very distinct, especially the atrial. Surface smooth and soft, slightly wrinkled. Colour from light-grey to pale horn tint, darker at the apertures. Length and breadth variable; as an average—length, 5 cm.; breadth, 4 cm.

Test thin, except at the base, where it is greatly thickened and has always gravel attached to or imbedded in it. Vessels large in the base, elsewhere few and of small size.

Mantle moderately muscular, especially on the tubes and down the centre of the right side. Tubes long, atrial much wider than branchial, which is bent towards the ventral edge in the middle of its length.

Branchial sac extending to the base of the mantle, not longitudin-

ally plicated. Transverse vessels all narrow. Papillæ only at the angles of the meshes, long, tapering, and curved like tusks. Broad membranes hang from the transverse vessels, and are stretched over the convex sides of the papillæ and attached to their apices. Three to five stigmata in a mesh.

Dorsal lamina very broad in its lower half, transversely ribbed, and minutely tuberculated at the edge.

Tentacles 35 to 40, long, crowded, their bases touching, of different lengths but not alternating.

Olfactory tubercle oval in outline.

Several specimens from Station 49 (South of Nova Scotia), 83 fathoms.

A few of the specimens are not so much depressed as the others, and have rather an oblong shape and terminal apertures.

This species shows considerable resemblance externally to *Ascidia obliqua*, but differs from it in the structure of the branchial sac, and especially in the form and arrangement of the papillæ.

Abyssascidia, n. gen.

Test cartilaginous, transparent. Branchial aperture about 12 lobed, atrial about 8 lobed. Attached by ventral surface.

Mantle thin. A few large distant muscle bands on the left side.

Branchial sac not longitudinally plicated.

Tentacles simple, filiform.

Viscera on the right side of the branchial sac, intestine small, stomach short and wide.

Reproductive organs forming a round mass situated on the right side of the intestinal loop.

Abyssascidia wyvillii, n. sp.

External appearance.—Shape oblong, rather pointed at the anterior end, rounded at the posterior end; attached to a small manganese nodule by the lower (ventral) surface in front of the middle; flattened so that, the branchial aperture being anterior, the atrial is on the upper surface three-quarters of the way to the posterior end, and rather to the right of the middle; in consequence of this, more of the left than of the right side enters into the formation of the upper

surface. Branchial aperture at the edge, slightly to the right of the anterior end, 12 to 14 lobed; atrial 8 or 9 lobed, both sessile. Surface smooth. Colour very light-grey, transparent. Length, 6 cm.; breadth, 4 cm.

Test thick, rather solid, transparent; no vessels. Consists of hyaline matrix and small fusiform cells, no bladder cells.

Mantle very thin, endostyle and viscera seen through distinctly. A few large distant muscular bands run round the right edge, and extend over the left side nearly as far as the endostyle. Atrial tube prominent and having fine muscle bands. Branchial also muscular, but not projecting.

Branchial sac large, fills the whole mantle cavity. Every second transverse vessel slightly larger than the intermediate ones; here and there the stigmata extend from the one large vessel to the other, cutting through the intermediate smaller one. The internal longitudinal bars widen at each intersection with a transverse vessel. Stigmata rather wide, three in each mesh. No papillæ at the corners of the meshes. Tusk-shaped ducts, to which horizontal membranes are attached, connect the transverse vessels with the swellings on the internal longitudinal bars.

Dorsal lamina reduced to a series of conical processes (languets).

Tentacles few, distant, small, and filiform. Two at each side of the anterior end of the endostyle, and a few others in the usual circle, but separated by nearly their own length from each other.

Viscera on the right side of the branchial sac, at the posterior end, relatively small.

Alimentary canal narrow. Œsophagus opens near the base of the branchial sac. Stomach short, wide, and barrel-shaped.

Reproductive organs forming a large rounded mass on the right side of the intestinal loop at the ventral end. The ovary occupies the centre, and the spermatic vesicles are arranged round the periphery. The oviduct and vas deferens emerge from the dorsal and posterior end of the mass, and course along the superior (anterior) margin of the intestine to their termination.

One specimen from Station 160 (South of Australia), 2600 fathoms.

This interesting form belongs undoubtedly to the Ascidiadæ

notwithstanding the large number of lobes round the apertures. It has affinities with *Ascidia* and *Corella*. The latter it resembles in the position of the viscera and in the shape and relative size of the intestine; the branchial sac on the other hand differs greatly from that of *Corella*, and exhibits the simpler structure found in *Ascidia;* while the membranes hanging from the transverse vessels and the languets replacing the dorsal lamina are exactly like the same parts in *Corella*.

Corella japonica, n. sp.

External appearance.—Shape longish ovate, the anterior end being narrower than the posterior. Attached by posterior end and half of left side; base sometimes prolonged into a few short tufts for attachment. Branchial aperture terminal or slightly on the right side of the extremity; atrial more than one-third of the way down and on the right side, nearer the dorsal edge than the middle. Both apertures sessile and inconspicuous. Surface slightly rough, especially at the anterior end. Colour grey. Length, 3 cm.; breadth, 1 cm.

Test thin.

Mantle very thin over most of the right side and lower half of left; while on the anterior end of the right side, the anterior (upper) half of the left side, and the tubes, muscular bands are extraordinarily developed, and attain a great thickness (up to 3 mm). Branchial and atrial tubes very muscular, the latter is the longer. Both apertures have ring-shaped ocelli of a light rust colour.

Branchial sac not folded. Internal longitudinal bars bear very long tapering papillæ. Stigmata curved, placed on the sides of conical infundibula set in square meshes. Secondary vessels coiled spirally, connected by a few radiating vessels. Broad horizontal membranes extend between the papillæ on the inner aspect of the sac.

Dorsal lamina in the form of languets.

Tentacles many, long, filiform.

Viscera on right side of branchial sac. Intestine large.

Two specimens from Yokohama, Japan, shallow water.

Several specimens from Kobé, Japan, 8 to 50 fathoms.

6. On some Applications of Rotatory Polarization. By Professor Tait.

Since last meeting of the Society I have found that Broch (in Dove's *Repertorium*) employed the combination of prism, Nicols, and quartz plate, for the purpose of measuring the rotatory power of quartz for different wave-lengths. I do not find, however, that he suggests its use for the determination of wave-lengths according to one definite standard. Nor does he seem to have used great thicknesses of quartz, which is essential to accuracy in the application I have proposed. In the *Annales de Chimie*, 1846, there is a translation of a part of Broch's paper, with the remark that the process, which he called a new one, was due to Fizeau and Foucault. Their paper, however, refers to quartz cut *parallel* to the axis; but it introduces the question, very important so far as my object is concerned, of the interference of two polarized rays after one has been retarded more than the other by very many wave-lengths. It is quite possible that this consideration, which I had for the time forgotten, may be found fatal to my method when very great thicknesses of quartz are employed on the bright lines given by glowing gases, for the purpose of estimating the velocity of the individual particles. It will not affect the method as applied to the spectra of auroras, comets, &c.

Prof. Niven (*Phil. Mag.* 1878) speaks of rotation of plane of polarization as the most delicate test of change of wave-length. It is so in theory, but in practice it cannot be compared to a train of prisms.

I have within the last fortnight operated with pieces of quartz from 4 to 8 inches in length, and have found that the sharpness of extinction of the red line of lithium is greater than that of the green line of thallium. The breadth of the latter must of course come more into play. The range of uncertainty for the orange sodium line is very much greater still. This was to be expected from its being double. With thick plates of quartz it cannot be extinguished at all. In fact, in order to extinguish it, a plate would be required which would make the difference of rotation for the two constituents one or more semi-circumferences. The least thickness of quartz for such a purpose would be somewhere about 13 feet, and about 500 successive oscillations of the luminous particles would

have to be strictly periodic. The experiment would be well worth trying, but it would involve great difficulties as well as considerable expense; and it might fail altogether on another account, viz., the *breadth* of the individual sodium lines.

I find it advantageous to replace the second Nicol by a double-image prism, and to take the reading when the two images of the slit are equally bright.

BUSINESS.

The following candidates were balloted for, and declared duly elected Fellows of the Society:—J. M. Thomson, Esq., King's College, London, W.C.; Dr C. G. Knott, Natural Philosophy Laboratory, University, Edinburgh; Dr J. A. Russell, Woodville, Canaan Lane, Edinburgh; W. W. J. Nicol, Esq., 15 Blacket Place, Edinburgh; Charles Prentice, Esq., 8 St Bernard's Crescent, Edinburgh; L. L. Rowland, M.A., M.D., Williamette University, Salem, Oregon, U.S.; Robert Pullar, Esq., St Leonard's Bank, Perth; The Rev. Professor Flint, Johnstone Terrace, Craigmillar Park; De Burgh Birch, M.B., C.M., 19 Albany Street, Edinburgh; J. Berry Haycraft, M.B., B.Sc., Physiological Laboratory, Edinburgh.

Monday, 15th March 1880.

The Right Rev. BISHOP COTTERILL, Vice-President, in the Chair.

The following Communications were read:—

1. The Topography of Jerusalem. By Lieut. Claude Reignier Conder, R.E.

The subject on which I have the honour of addressing you this evening is one far more complicated and difficult than that of the paper which I read to the Royal Society of Edinburgh some short time since. We have to deal, not with the surface of a country and the position of places of which the ancient names are still extant, but with a ruined city, buried to a depth of from 30 to 50 feet in rubbish on which modern buildings having been erected, and with

a topography in which there is scarcely a single important point which has not been controverted by one or more well-known writers.

The topography of Jerusalem has, moreover, formed the subject of works of every century from the age of Josephus to the present time, and can only be rightly understood after the study of about one hundred standard accounts of the city in all ages. Ruins, when discovered, must not rashly be assumed to belong to a period of great antiquity, since we know that even after the time of Herod the Great, magnificent buildings were erected by Hadrian, by Constantine, by Justinian, by the early Khalifs, and by the Latin kings, as well as by the Moslem rulers of the thirteenth, fourteenth, and fifteenth centuries.

Many relics which were at first thought to belong to the pre-Christian history of Jerusalem, have been shown, after careful examination by experienced architects, to be remains of the work of the later builders above cited. The subject of this paper must be confined, therefore, to an account of recent discoveries, and to an attempt at showing the bearing of such discoveries on the points of most general interest. The restoration of the ancient topography has, I take it, but small interest in itself except for a limited circle; but the questions which have given importance to the study are, I believe, very generally understood; and the controversies on Jerusalem topography appear to gather round two principal centres of interest—namely, the position of the site of Calvary and the Holy Sepulchre, and the position and the monuments of the Temple enclosure. It is proposed to show in what degree recent explorations have thrown light on these two central questions.

A new era in the history of Jerusalem research dates from the execution of the Ordnance Survey in 1864. This survey, undertaken partly at the expense of the Baroness Burdett Coutts (for the purpose of reporting on the water supply of the city), was executed by Captain (now Lieutenant-Colonel) C. W. Wilson, R.E., with a staff of non-commissioned officers from the Royal Engineers and was published by the Ordnance Survey Office at Southamption. The survey includes a map to the scale $\frac{1}{2500}$ of the city itself, with a smaller map of the envirous $\frac{1}{10000}$, and with a plan of the Temple enclosure $\frac{1}{500}$ as well as a number of special plans, of the Holy Sepulchre Church and other important buildings. The actual

levels of all the important points in the city were determined by instrumental measurement, and fixed definitely by connection with the line of levels which Colonel Wilson carried across from the Mediterranean to the Dead Sea.

This great work forms the basis on which all subsequent exploration rests, and supersedes all previous maps, such as those of Robinson, Vandevelde, Symonds, Catherwood, Barclay, &c., which, though invaluable at the time when they were constructed, are but imperfect attempts in comparison with the complete plan which is now available for students.

Colonel Wilson was not charged with any extensive commission for the purpose of excavations, and the few which he undertook did not lead to any important results. He was, however, the first explorer who gave a careful and minute account of the masonry of the great walls which surround the Haram or "Sanctuary," which is recognised as the site of the Jewish Temple enclosure.

The famous excavations conducted by Captain (now Lieutenant-Colonel) Warren, R.E., for the Palestine Exploration Fund, were commenced in 1867, and carried on until 1870. They resulted in discoveries of crucial importance, especially with regard to the extent and antiquity of the Haram rampart walls, and respecting the ancient contour of the site of the city and of the Temple.

The 1872 excavations were carried on at the expense of the German Government, in the very heart of the town, on the site of the old hospital of the Knights of St John. They also yielded very important results in the discovery of a great valley 100 feet deep, the existence of which had previously been denied by Canon Williams and other writers.

During the years 1872, 1873, 1874, and 1875, I spent several months of each year in Jerusalem, and was able to supplement the explorations in one or two particulars. I also collected, personally and by aid of residents, a large number of new observations as to the depth of the debris, which, when added to the measurements of Colonels Wilson and Warren, are sufficient to justify the tracing of contours over the entire site of the city, showing the ancient surface now hidden by rubbish, and thus defining the great natural features described by Josephus, which are almost obliterated by the gradual filling in of the original valleys.

A single instance of the value of such explorations may not be out of place here. Dr Robinson, the famous American traveller, discovered at the south-west angle of the Haram enclosure, the haunch stones of an ancient arch, and identified them as belonging to the bridge leading from the royal cloister to the upper city. Canon Williams challenged this assumption, and stated his opinion that Josephus should be understood to refer not to a bridge but to an embankment. The controversy extends over about twenty pages of print.

A single mine of Colonel Warren's set the question at rest, by the discovery of the great west pier of the ancient bridge, and of the voussoirs lying on the pavement 42 feet below the present surface, proving the existence of a magnificent viaduct 80 feet high with arches 42 feet span. Not content with this discovery, Colonel Warren broke through the pavement and sunk his shaft still 20 feet before reaching the rock, where, jammed in the channel of a rock-cut aqueduct, he discovered the voussoir of a yet older bridge, which had been overthrown before the pavement was constructed on an accumulation of 20 feet depth of rubbish. The earlier bridge is believed to be that mentioned as having been broken down at the time of Pompey's siege of Jerusalem; while the second viaduct, constructed by Herod the Great, was standing in the time of Christ, and was overthrown during the great siege of Titus.

Colonel Warren's explorations included a fairly complete examination of the southern, eastern, and western walls of the great enclosure of the *Haram* or "Sanctuary," and an examination of the passages, cisterns, and vaults in the interior.

The *Haram* is a quadrangle containing 35 acres, the interior surface roughly levelled, being partly rock, partly supported on great vaults, and partly filled in with earth, behind the great rampart walls. The four sides are of unequal length, the shortest wall being that on the south, 922 feet long. The south-east angle measures 92° 30,′ and the south-west is a right angle. The east wall is 1530 feet long, the west wall 1600, and the north 1042 feet.

In the north-west corner the rock has been cut down on the interior so as to leave a great block, 40 feet high, standing above the court, and now occupied by barracks. This rocky citadel measures 350 feet along the north wall of the Haram and 100 feet north and

south. The north face of the scarped block rises above an artificial trench, 50 feet deep and 140 feet wide, separating the Haram enclosure from the hill to the north.

The existence of a great viaduct at the south-west angle (identified with a bridge mentioned by Josephus) has already been mentioned. At the south-east angle, Colonel Warren made two further discoveries, both of which are of the highest value.

The great rampart wall here rises 160 feet from the rock foundation, of which height, however, only the upper half is at present visible above the surface. The ancient masonry is here found extending from the foundation almost to the top of the wall, and Colonel Warren found on the six lowest courses Phœnician letters, painted with a red pigment, which appear to have been intended to denote by a letter or numeral the course for which each stone was designed, beginning with the foundation course.

The second, and yet more important, discovery made by Colonel Warren at this point was that of a great rampart wall extending southwards from the Temple wall at the south-east corner. The two walls abut together with a straight joint, which extends from top to bottom without any bonding stones, indicating that they were probably built at different periods. The newly discovered wall was traced southwards for nearly 800 feet, and although it is nowhere visible above the surface, was found to be standing beneath the rubbish to a total height of about 70 feet. A great projecting tower was explored along the course of this wall, 400 feet south-west of the Haram corner, and there can be no reasonable ground for doubting that Colonel Warren was right in identifying this magnificent fortification with the city wall which Josephus mentions as protecting Ophel, while the great projecting tower answers in position to the "tower that lieth out" mentioned in the Book of Nehemiah also in connection with the Ophel wall.

The masonry of which the Sanctuary walls are composed may be divided into three principal varieties, belonging to distinct epochs of the history of the structure. First come the magnificent drafted stones, of gigantic proportions, forming the foundation of the wall and extending upwards generally higher than the present surface. It is undisputed that this masonry belongs to the period prior to the great destruction of Jerusalem by Titus. In the second place,

there is a distinct style above the drafted masonry, consisting of stones of large and remarkably square proportions without any draft. These stones are recognised by architects as belonging to the early Byzantine period. In the third place, there is a patchwork of smaller masonry, forming the upper part of the ramparts, and composed of Crusading, Saracenic, and Arab restorations, dating from the twelfth century to the present time.

It is with the two first classes of the masonry that we are at present concerned, and more especially with the drafted style.

The average height of a course of the drafted stones is about 3 feet 6 inches. The blocks vary considerably in length, the longest as yet known being 38 feet 9 inches and the second longest 23 feet 8 inches. The draft or sunk channel surrounding the four edges of the face of each stone is about 3 inches wide. The faces within this channel are generally dressed smooth, and project about half an inch in the best preserved specimens; but in the lower courses, where the stones were never exposed to view, the boss or raised face within the marginal draft is left undressed, and projects in some cases as much as 18 inches. The rampart walls are built with a batter or sloping face, and the draft allowed of great precision in the alignment of the stones even when the boss was left rough, the batter being obtained by setting back each course about 2 inches from the face of the course on which it stood, the measurement being taken from the face of the marginal draft.

Beyond the fact that this magnificent masonry belongs to the ancient Jewish Temple, no definite conclusion has as yet been generally accepted respecting its date. Colonel Warren has distinguished different classes of the masonry according to the finish of the stones, and refers these to different periods of construction. He attributes the stones at the foot of the east wall, close to the south-east angle, to the time of Solomon, while the unfinished masonry on the south-west he refers to Herod the Great. The well-known French explorer and architect, the Duc du Vogüé, is of opinion, on the other hand, that the whole of the drafted masonry at present existing is attributable to the time of Herod the Great, and although Colonel Warren's conclusions are generally well worthy of attention and singularly shrewd, there appear to me in this case to be many indications favouring the opposite view.

It is indisputable that the presence of Phœnician masons' marks at the foot of the great wall is proof of the antiquity of the structure; but there are good reasons for doubting whether Solomon's Temple enclosure can have extended so far south, while it is certain that similar characters would have been employed by native masons in the time of Herod the Great.

The masonry of the east wall of the Sanctuary towards the north is, moreover, of a somewhat different character and material from that of the south-east angle, and is attributed by Colonel Warren to a later epoch; yet, on this masonry also, red paint letters similar to those previously mentioned were found on the foundation stones of the wall.

There is, moreover, an indication of great value as to the date of the masonry yet to be noticed. The dressing of the stones is distinctive, and has not as yet been found elsewhere in Syria. An eight-toothed chisel was driven along the draft, and again used at right angles to its former direction, thus producing a regular criss-cross pattern on the surface of the stone. This dressing is found on each of the three ancient walls of the Haram, and on masonry of various degrees of finish, seeming to indicate a common period of execution for all the varieties of masonry, for it would be a bold assumption to suppose that the masons of Solomon used a chisel of exactly the same dimensions and number of teeth employed by Herod's masons, considering that a lapse of time equal to that which separates the reigns of Alfred and Victoria occurred between the two building epochs in question. The criss-cross dressing is found on the voussoirs of the Tyropœon Bridge, which, as before explained, has been found leading westwards from the Sanctuary wall, and which is plainly attributable to the Herodian period, to which it would therefore seem all the masonry similarly dressed is most naturally assigned.

The second type of masonry above the drafted stones has been attributed to Justinian by the same architectural authority above quoted, the Duc du Vogüé. Justinian is known to have erected splendid buildings on this site, and is the only builder between Herod and Omar who is historically recorded to have constructed anything on this side of Jerusalem. The architectural details accompanying the large smooth masonry in question are, moreover, identical in character with the style which is found throughout Syria in buildings of the fifth and sixth centuries, A.D.

Next in importance to the exploration of the Sanctuary ramparts must be ranked that of the subterranean passages and chambers within the enclosure, which were most carefully examined by Colonels Wilson and Warren, and the most important of which I have also frequently visited.

Two great passages lead from the two ancient gateways in the southern walls; two others lead from similar entrances on the west. The first two portals are each double, with an internal vestibule supported on pillars. The western gates are single, and the passages are half the width of the former two. The gateways in each case are ancient, with massive lintels above, having marginal drafts round the edges. The masonry of the passages, however, is in every case of more modern character, apparently belonging to the period of restoration under Justinian.

In addition to these vaults, there are no less than thirty large cisterns within the area, the aggregate capacity of which is calculated at about ten million gallons. Most of these great tanks are rock-cut, and some, which are closed at the ends with masonry and cemented inside, seem originally to have been passages like those above mentioned, but have been subsequently utilised as cisterns.

The measurements taken in the mouths and roofs of these cisterns have served to define generally the original rock surface of the ridge enclosed within the ramparts of the Sanctuary—a narrow spur running north and south with steep western slopes and more gradual eastern declivities. The rock at the north-west angle of the enclosure, standing 40 feet above the inner court, dominates the whole enclosure; but the ridge rises gradually to a point near the centre of the Sanctuary, where a rough rock surface is exposed beneath the beautiful "Dome of the Rock," at a level about 20 feet higher than that of the average surface of the enclosure. The broadest and flattest part of the ridge is found in the immediate neighbourhood of this rock, which forms the top of the hill included in the area. The level of the crest falls gradually southwards towards the tongue of land called Ophel, south of the Sanctuary; and the lowest point of rock within the area is at the south-east angle, where the foundation of the wall is 160 feet below the top of the *Sakhrah*, or sacred rock visible in the Dome of the Rock.

Such, briefly described, are the leading facts recovered with regard

to the Sanctuary at Jerusalem, so far as they have reference to the Jewish Temple. Briefly ennumerated they are as follows:—*First*, The existence of ramparts of gigantic masonry plainly attributable to the Jewish period and probably to the age of Herod the Great. *Secondly*, The recovery of a rock citadel with an outer fosse at the north-west angle; of an ancient viaduct of the Herodian period at the south west-angle; and of the great Ophel wall abutting on the Sanctuary at the south east-angle. *Thirdly*, The existence of two southern gateways of the Jewish period and of two western portals, in addition to the entrance over the bridge and other passages in the interior of the area. *Fourthly*, The determination of the rock-levels throughout the area, and the fact that the Sacred Rock occupies the culminating point of the broadest part of the Temple ridge.

These facts have all more or less important bearings on the great question of the restoration of the Temple enclosure, which is one of the most important subjects of controversy in the topography of Jerusalem. Authorities are at present divided into two parties, the largest of which recognises in the present Sanctuary enclosure, as a whole, the area of the Jewish Temple as restored by Herod; while the smaller party, following the teaching of Mr James Fergusson, supposes that Herod's Temple occupied only a square of 600 feet side in the south-west portion of the area.

The reason for this last assumption is the statement given by Josephus, that Herod's Temple enclosure measured a furlong on either side—approximately 600 feet; and until it had been ascertained that the eastern wall, and the northern and eastern parts of the west and south walls of the Sanctuary (wherever examined) were of antiquity equal to that of the ramparts towards the south-west part of the area, such a theory had many points in its favour. The recovery of the great Ophel wall has, however, proved to be the most important of the many valuable discoveries made by Colonel Warren, because Josephus has clearly stated that the Ophel wall joined the *east* wall of Herod's Temple, just as the rampart now found joins the east wall of the present Haram. The existence of a rock citadel and fosse on the north-west answers also exactly to the account which Josephus gives of the citadel of Antonia, which dominated the Temple courts; and three angles of the ancient enclosure are thus identified with corresponding angles of the

modern area, through the recovery of remains of a citadel, a bridge, and a city wall, described by Josephus.

The recovery of the north-east angle is, however, not as yet complete, although Colonel Warren has been able to throw light on this question also. The north wall of the present enclosure is generally acknowledged to be later than the other three, and consists of very inferior masonry. The east wall has, moreover, been proved to run northwards without any break beneath the surface, beyond the present north-east angle of the enclosure; but, as already mentioned, this part of the east wall is of inferior material and finish though marked as ancient by the existence of the ancient red paint letters. It seems probable that this wall formed part of the rampart erected by Agrippa about 44 A.D. on the north side of Jerusalem, which wall joined the east rampart of the Temple enclosure. There are indications, which cannot now be given at length, tending to show that the old north-east angle of Herod's enclosure was situated about where the Golden Gate (an edifice of the Byzantine period) now stands, and that an area of about $2\frac{1}{2}$ acres in the north-east portion of the present enclosure was included within the boundaries at a period later than that of Herod's Temple.

The above remarks apply exclusively to the Second Temple as restored by Herod the Great. Although it is certain that the Holy House itself and the altar occupied the same spot in the time of Herod on which they were first reared by Solomon, the extent and position of the surrounding courts as they existed in Solomon's time are as yet entirely unknown, no certain remains of that period having been recovered, and no definite accounts of their measurements being extant. We know that great alterations were effected by Herod; that he increased the area (Josephus says in one passage that he doubled it) and that he took away ancient foundations and laid others. Considering the lapse of twenty-nine centuries, and the alterations deliberately effected at the late period, it seems improbable that we should succeed in restoring the Temple of Solomon, though there seems no reason why the main features of that of Herod should not be recovered with certainty, in the process of explorations, which are still both necessary and possible.

A second objection to the proposed restriction of the Herodian Temple within a square of 600 feet side in the south-west portion

of the existing area, arises when the levels of the rock surface are carefully studied.

The Temple enclosure consisted of three principal courts, rising in successive steps towards the great fane which stood, as Josephus states, on the top of the hill. In order to fit such a building to the ground, within the present area, it is necessary to start with the assumption that the culminating point is to be found in the Holy Rock—the present top of the Sanctuary Hill. If we place the Holy House over this rock, the levels of the various courts agree exactly with the ascertained levels of the rock as at present remaining; but if we were to place the Holy House further south-west it would have stood, not on the top, but half way down the steep western slope of the mountain. The lowest court would be found to occupy the highest part of the hill; and foundations varying from 50 to 90 feet in depth would become necessary on the supposition that the great edifice was built up from the rock.

The restoration of Herod's Temple on the supposition that the central fane stood above the sacred rock, called *es Sakhrah*, in the middle of the present Dome of the Rock, has occupied my attention for more than seven years past; and the indications which confirm this restoration are perhaps sufficiently interesting to claim a detailed enumeration.

In the first place, The rock in question has been regarded by Jews, Christians, and Moslems, for at least fifteen centuries as the site of the Holy of Holies. We learn from the Talmud that a stone or rock called "Foundation" formed the floor of the most holy place, and that it was regarded as the foundation of the whole earth. From early Christian writers we gather that this sacred rock was venerated by the Jews in the fourth century, and the description then given tallies with the present Sakhrah. The traditions of the Crusaders and the Arabs reproduce those of the Jews in regarding the present Sakhrah as the foundation stone of the world, and thus serve to connect the present sacred rock with that on which the Temple stood.

Secondly, The Temple faced eastwards, and its door, according to the Talmud, was directly opposite the summit of Olivet. A line drawn due east through the Sakhrah rock will be found, if produced, to strike the top of Olivet, which would not be the case were the Holy House placed further south.

Thirdly, The levels of the various courts can easily be deduced from the Talmudic account of the Temple; and as the floor of the Temple is fixed at the level of the Holy Rock, the levels of the surrounding areas may be compared with those of the present enclosure. In every case the result is satisfactory The Court of the Priests ought, on the present theory, to have been 2432 feet above the Mediterranean, and an observation of the rock at exactly that level has been obtained within its area, the level being only a foot beneath the present flat surface of the court round the dome of the rock. The Court of the Women should have a level 2429, and it is almost certain that the rock is nowhere above (nor very much below) that level within its precincts. The Court of the Gentiles should be 2411 above the sea, which is the average level of the present surface outside the platform or court round the Dome of the Rock. Several other exact results might be given, but the preceding will be sufficient to show how well the ancient Temple may be fitted to the ground surrounding the Holy Rock.

Fourthly, There were no cisterns within the limits of the inner courts of the Temple; and none of the great cisterns which exist in so many other parts of the Sanctuary enclosure come within the limits of the courts according to the present restoration.

Fifthly, A subterranean gallery leading to a subterranean bath-house ran northwards from the great gate Moked on the north-west side of the Priests' Court; and such a gallery with an adjoining vaulted chamber is found in exactly the required position according to the present restoration.*

It should be noted that the arrangement of the courts which has been followed is that given in the Talmud, which agrees fully with the more general account of Josephus; and that the cubit is assumed to have been about 16 inches in length,—a determination which, as I had occasion to notice in a former paper, is based from a comparison of Talmudic accounts with existing monuments, especially the Galilean synagogues and the masonry of the rampart walls of the Sanctuary above described.

If the above views should be found tenable, discoveries of the

* The identification of these vaults with the passages mentioned in the Talmud, is due to Colonel Warren, whose plan of the Temple is, however, somewhat different from that proposed in this paper.

highest interest may perhaps still await us. It is known that the pavement of the platform round the Dome of the Rock is partly supported on vaults as yet unexplored, and that there are rocky scarps still to be examined in other parts of the same area. It seems quite possible that the rocky foundations of Herod's Temple may still lie hidden beneath the modern pavement; and the progress of exploration during the last twenty years has been so steady and rapid, that it is perhaps not unreasonable to hope that the secrets of this hidden portion of the sacred area may yet be unfolded, and the position of the Holy House and the Court of the Priests fixed by the actual recovery of the foundations at present covered by a modern pavement.

While residing at Jerusalem I entered every chamber and cistern which could be reached in the immediate vicinity of the Holy Rock. I was fortunate in the discovery of a rock scarp previously unnoticed, but there is no doubt that substructures remain still to be explored especially towards the east of the present platform.

Such seems to be the general result of the successive explorations of the High Sanctuary at Jerusalem, so far as Jewish antiquities are concerned. It is now proposed to consider the results of exploration in the city itself, in special connection with the question of the true sites of Calvary and the Holy Sepulchre.

The first requisite for a satisfactory restoration of the ancient topography of the city is a clear understanding of the natural site on which it was built. The hills and valleys have been rendered almost indistinguishable through the accumulation of rubbish; and modern explorers have therefore agreed that the first and most important object to be kept in view is the determination of the level of the rock-surface in all parts of the city.

The Ordnance Survey of Colonel Wilson not only gave the means of easily determining such levels by reference to fixed bench marks above the surface, but also laid the foundation of the inquiry by marking the rock wherever it occurred on the surface. The number of observations added by Colonel Warren and by others, including my own measurements, has given a total of 265 observations within an area of about 250 acres occupied by the ancient city; and we are thus able to run contours and draw sections which approxi-

mately determine the original surface within very narrow limits of error, and which render the relative positions and elevations of the principal features as certain and definite as is necessary for antiquarian purposes.

The most important discovery resulting from these researches is that of the great valley which occurs in the very heart of the city, having its head not far from the Jaffa Gate. The rock appears in the Church of the Holy Sepulchre in two places at a level 10 feet above the floor, but just south of the church there are vaults 18 feet deep.

The rock is also known in many places along the top of the hill called Zion by the modern Christians, but between these two eminences it is never visible on the surface, which is somewhat depressed.

Dr Robinson pointed out that there was apparently a valley separating the southern hill from that on which the Church of the Holy Sepulchre stands. Yet this was so little capable of proof before the levels of the Ordnance Survey had been taken, that Canon Williams did not hestitate to affirm that no such valley existed.

In 1872 the excavations on the site of the Hospital of St John resulted in the discovery of magnificent vaults, 50 feet deep and 200 feet long, which formed vast reservoirs for the supply of the Hospital. These were cleared out to the bottom, and the rock was found at a level 60 feet below the top of the Holy Sepulchre hill and traced all along the vaults.

In 1876 another vault was found further east, measuring 120 feet north and south. The rock floor was found to fall rapidly southwards, and the slope of the north bank of the great valley was thus defined over a considerable section. In 1870 Colonel Warren had made observations which define the position of the south bank, and the number of observations in and near the valley now number about thirty in all. The general result is, that its course is traced eastwards to the Haram, where it joins a narrower valley running north and south from the Damascus Gate. The newly recovered valley is 60 feet deep and 600 feet wide north and south.

The existence or non-existence of this important natural feature used to form one of the favourite subjects of Jerusalem research. Those who, following Dr Robinson, continued to believe in its

existence were stigmatised as *Tyropœonists*, from the theory that this was the course taken by the valley which Josephus calls the Tyropœon. But not even the party which now proves to be right was prepared for the great width and depth of the valley.

Having thus recovered the most important of the lost physical features of the site, we are better able than of old to understand the description given by Josephus, which is almost too well known to need repetition. Josephus speaks of three hills and two valleys within Jerusalem. First, of the great square hill, on which the upper city stood in his time; which he identifies with the citadel of David's time, called in the Bible the "stronghold of Zion." The lower city occupied the slope of Akra—a hill less elevated than the former, having a gibbous or bulging shape, and divided by the Tyropœon valley from the first mentioned hill of the upper city. Another valley separated Akra from a third and lower hill, which was called Bezetha, and which was situated north of the Temple hill, with an artificial trench cutting it off from the Antonia citadel.

All these features are recovered. The elevations all prove to be relatively those described by Josephus, the shapes of the hills, the dividing valleys, even the artificial rock-cut trench, are found correctly to describe existing features.

The large square hill, which Josephus incidentally identifies with Sion, is that now called by the same name. The flat plateau on the summit is 2530 feet above the Mediterranean. North of the great dividing valley a spur, 40 feet lower than Sion, is found bulging out eastwards. It is divided from the hill north of the Temple by a second valley which joins the first; and the positions of Akra, Bezetha, the Upper City, and the Tyropœon, are in the opinion of Colonel Warren, and I may be perhaps permitted to add in my own opinion also, now defined in the relative positions indicated some forty years since by Dr Robinson.

The course of the ancient walls which surrounded Jerusalem is carefully described by Josephus. On the south-east and west the city was defended by deep valleys, but on the north there were three consecutive lines of defence at the time of the great siege. The remains of these ancient fortifications seem, however, for the most part to have disappeared, and the only traces which we may con-

fidently expect to recover are the rocky scarps which formed the base of the walls in various parts.

The modern walls of Jerusalem are partly composed of ancient drafted masonry, which is, however, not *in situ*, and there seems good reason to suppose that the materials of which the present fortifications are composed were taken from the ancient walls. The ramparts have been destroyed and rebuilt seven times since the siege of Titus, and the disappearance of all traces of the ancient third wall is most easily explained, on the supposition that its stones have been removed, since there is no great accumulation of rubbish to hide any remains which might exist north of the city.

Even within the last half century many relics of the ancient city have been lost for ever. Dr Robinson speaks of the remains of towers north-west of the city, which have now entirely disappeared beneath modern buildings. There can be little doubt that these represented the course of the third wall, and the careful measurements and angular observations of Dr Robinson are thus of the highest value. In 1864 other remains were noted, during the execution of the Ordnance Survey, of ancient masonry, which has since been broken up by the peasantry. The accumulation of rubbish in Jerusalem has in fact been of the greatest service to antiquarians, and where no rubbish exists the ancient buildings have been entirely destroyed.

The first wall of Jerusalem defended the upper city. At the north-west angle was a fortress with three famous towers, Hippicus, Phasaelus, and Mariamne, which stood on solid bases and protected Herod's palace.

There can be little doubt that the present citadel south of the Jaffa Gate marks the site of this fortress. The great tower now called David's Tower has been proved to stand on a solid base. Its dimensions are almost exactly those given by Josephus for Phasaelus, and it resembles that tower also in having an outer platform with a battlemented wall. The north-west tower of the citadel may mark the site of Hippicus, but the present structure is rather larger than the tower which Josephus describes as the corner tower of the first wall on the north-west.

The modern citadel is surrounded by a fosse, and east of this is the market place standing on arches, the rock being 30 feet below the surface of the street.

The vaults are entered by a door from the fosse, but this is now built up. The examination of these vaults would be an undertaking of the greatest interest as tending to throw light on the course of the first wall.

From the corner tower Hippicus the wall ran east to the Temple. The rock levels now obtained show the existence of a precipice or scarp running eastwards from the vicinity of the citadel; and on this line, also, the foundations of two ancient towers have been discovered, which seem to have formed part of the north face of the wall.

The south-west angle of the ancient city is now recovered in a satisfactory manner. A rocky scarp, which had long been observable, was thoroughly explored in 1874 by an English engineer, Mr. Henry Maudslay. During my stay in Jerusalem I made a careful survey of the remains discovered. The rock was found to have been worked to a vertical face to a height of 50 feet for a distance of 150 yards. At either end of this scarp was a projecting rock buttress, the base of a tower 40 feet square. A flight of rock-cut steps led up to each tower from the fort of the scarp, and numerous cisterns were excavated in the rock on the top of the tower bases.

It is interesting to note that this arrangement agrees exactly with the description which Josephus gives of the towers along the wall of the ancient city.

The rock scarp is found to continue beyond the towers both northwards and eastwards. The towers stand in the precincts of the Protestant cemetery and bishop's school, and Mr Maudslay was unable to obtain leave to continue his researches beyond the limits of this property. There can be little doubt that one of the most useful and interesting researches remaining to be undertaken, consists in the following out of this discovery, and the further tracing of the ancient wall foundation.

The manner in which the first wall joins the Temple enclosure at the south-east angle has already been described. It is not, however, as yet known exactly where the wall crossed the Tyropœon valley. The account given in the Book of Nehemiah and that of Josephus are both too indefinite to be clearly understood without the aid of explorations along the line. A careful tracing eastwards

of the scarp already recovered might, however, probably result in the settlement of this question.

The second wall is very briefly described by Josephus. It started from a certain gate in the first wall called Gennath, which, appears to have been at no great distance from the tower Hippicus and it ran thence *in a curve* to Antonia, enclosing the lower city. The gate Gennath has not yet been found, and this also is one of the great future objects of research. No certain relics of this wall indeed have been as yet recovered, for the remains of drafted masonry which Canon Williams found east of the Church of the Holy Sepulchre have been very carefully examined, and prove to have belonged to a building which the Duc du Vogüé has shown to have been part of the ancient Basilica of Constantine, built in 335 A.D. over the supposed site of the Holy Sepulchre. The northern scarp of the rock-cut fosse which separated the tower Antonia from the northern hill of Bezetha has been traced westwards for some distance. It seems very probably to have formed the counterscarp of a ditch outside the second wall. Colonel Warren also found remains of a rocky scarp facing northwards within (or south of) the line of the above mentioned counterscarp, but this investigation is as yet incomplete, and a shaft is much needed within the precincts of an open plot of ground immediately west of the scarped rock of Antonia.

The materials of which the third wall was composed seem, as already remarked, to have been removed by the builders of the later walls of the city. The general line of this wall, which was built about 44 A.D. by Herod Agrippa, has been laid down by Colonel Warren in a manner which appears to me to be satisfactory.

Starting from Hippicus this wall ran out northwards to a certain large octagonal tower called Psephinus, which stood on ground so high as to command a view of the mountains of Arabia. The remains of towers were discovered by Dr Robinson along this part of the course of the wall, but are now hidden or destroyed. From the high ground at the point now occupied by the Russian Cathedral, the mountains of Arabia east of Petra were distinctly visible when covered with snow in the winter of 1873–4.

From the tower Psephinus the wall ran east and then south-east and passed over certain caverns called the "Caverns of the Kings."

Most modern writers identified these caves with the great quarries which extend under Jerusalem immediately east of the Damascus Gate. These quarries were those whence the Temple masonry was hewn, and a curious rude carving of a figure resembling the Assyrian winged bulls has been found in them and is now in England.

From this point the line of the third wall seems to have coincided with that of the present north wall of Jerusalem, standing on a rocky scarp with a rock-cut fosse outside. The modern eastern wall, as far south as the Sanctuary enclosure, also appears to be on the same line occupied by the east face of the third wall, which thus joined on to the comparatively roughly finished wall which, as above mentioned, runs northwards beyond the present north-east angle of the Sanctuary.

The main interest of tracing these ramparts lies in the connection of their course with the question of the genuineness of the site now shown as representing the Holy Sepulchre and the Hill of Calvary. It is admitted that if the remains of the second wall can be shown to have included these sites within the boundary of the then existing city, the description of the position of Calvary outside Jerusalem—as plainly set forth in the New Testament—would not be fulfilled. The question, however, of the course of the second wall is still unsettled.

Without wishing to enter into this old and fierce controversy at the very end of my paper, I would point out three indications which arise out of the recent discoveries.

First, The hill on which the present Church of the Holy Sepulchre stands is now identified, apparently beyond dispute, with that of Akra or the lower city, which was encompassed by the second wall.

Secondly, The deep valley separating this hill from that on the south runs up almost to the Jaffa Gate. The second wall started from some unknown point on the north side of the first wall and ran in a curve to Antonia. It seems impossible to suppose that it can have *crossed through* the great valley, and if it was built on the high ground at the head of that valley, and ran thence in a curve to Antonia, it must apparently have included the Holy Sepulchre Church.

Thirdly, The site of the church is beyond dispute within the compass of the third wall, which was built to protect suburbs which had extended beyond the second wall. It is true that the second

wall only existed in the time of Christ; but the third wall was built only ten years after the Crucifixion. It seems difficult to believe that the suburb, in the short period, had extended itself over 120 acres, so as nearly to double the area of Jerusalem. It seems more probable that at the time of the Crucifixion the site now shown as Calvary was already, if not within the walls, at least far within the limits of the existing town.

To state briefly the objection, raised first in the eighth century and repeated by various writers in almost every succeeding age, the site now shown as representing Calvary is so nearly in the middle of Jerusalem, that it seems impossible, on any reasonable reconstruction of the ancient city, to suppose that at the time of the Crucifixion it was outside the border of the inhabited town.

One of the strong arguments in favour of the genuineness of the site has always been found in the existence close to the Holy Sepulchre of an indisputably ancient Jewish tomb. I propose to inquire, therefore, in conclusion :—First, What is this tomb, and how is its presence inside Jerusalem to be understood? Secondly, If the site of Calvary was not in reality where it is shown, where it is likely to have been?

As regards the tomb, it is a chamber cut in rock, with nine *kokim* or graves, of which three are placed at a lower level, sunk in the floor of the chamber. The fact that the graves are *kokim*, that is longitudinal tunnels, running in from the sides of the chamber, so that the body lay with its feet towards the chamber and its head away from it, and that they are not *loculi*, or graves placed sideways on the walls of the chamber, proves not only that the tomb is Jewish, but that it belongs to an early Jewish period previous to the time of Christ.

We are informed by the Talmudic writers that all the tombs were outside Jerusalem, except the tombs of the nine kings of Judah, and another tomb of the prophetess Huldah. Josephus tells us that the graves of these kings were hidden, so that even those standing inside the monument could not see them. The site of the Tombs of the Kings has long been anxiously sought, for the present traditional site is recognised as having been invented in the fifteenth century. The ancient tomb above described answers all the requirements of the tombs of the kings.

1*st*, It is an ancient Jewish tomb.

2*d*, It is within Jerusalem.

3*d*, It contains graves for nine kings, which was the number buried, including David and Solomon.

4*th*, It is the only known Jewish tomb inside the city, ancient or modern.

This view, which is, I believe, original, has already been cordially accepted by many students of the question. It thus furnishes an argument against instead of in favour of the present site of the Holy Sepulchre.

As regards the probable site of Calvary, I have also in conclusion to mention a new indication.

It is agreed that Calvary and the Holy Sepulchre were close together and outside the town, and it is generally supposed that Calvary was the place of public execution.

The tomb of Joseph of Arimathea, where Christ was laid, was in a garden, as are still the tombs of wealthy personages, but it was not the less likely to be near the great cemetery of the town. After careful investigation, and the recovery of inscriptions, frescoes, sarcophagi, and other remains, it has been pretty clearly shown that the ancient Jewish cemetery of Jerusalem was on the north of the town. The southern cemetery is Christian, and there are very few ancient Jewish tombs on the east. On the north, among the gardens which still extend over the flat ground, as described by Josephus, there are many ancient tombs, including that of Simon the Just. It is in this direction apparently that the Holy Sepulchre should be sought, though it is probably now beyond the power of modern research to identify out of so many sepulchres that of Joseph of Arimathea.

Calvary was, we may perhaps assume, the place of Jewish public execution. The recovery of the place of execution is therefore a matter of the highest interest. In the Talmud the place is described under the name "House of Stoning," as being just outside the city. It appears to have been a precipice some 12 feet high, over which the culprit was thrown before the first stone was cast at him. The site of this place is still pointed out by the Jews. It is a precipice with a swelling mound or hill above, and a cavern in the cliff, which is known to Christians as Jeremiah's Grotto. The

site in question is just outside the Damascus Gate, and can be easily shown to have been outside even the third wall of ancient Jerusalem.

There seems no doubt that this is the ancient place of execution, and I leave it to your consideration, whether it may not reasonably be supposed to be the site of Calvary, "the place of a skull."

In conclusion, I would call your attention to the immutability of the topography of the Holy City. The Rock of Foundation still stands in a Temple, and within a sacred enclosure. The citadel of Antonia is still a citadel. The Upper and Lower Markets are still markets. The Royal Towers still form the western fortress of the town. The venerated tomb of the kings of Judah is surrounded by a famous cathedral; but the site of Calvary—the place of execution—is only graced by a cemetery with an adjoining slaughter-house; and the private sepulchre of the rich man is still indistinguishable among the number of rich men's tombs in the gardens beyond the city walls.

If there are any here interested in the subject of further exploration at Jerusalem, I would point out that much yet remains to be done. The second wall has to be found. The great discoveries on Ophel and Sion require to be followed up, the secrets of the enclosure of the High Sanctuary are not exhausted, and mines are much needed in the ground immediately west of Antonia.

It is my earnest hope some day to be enabled to take up the task of excavation from which such great results were obtained by the energy and skill of Colonel Warren, and I trust that should such an opportunity arise the funds may not be wanting; for the difficulties arising from the dangers of the work were counted as but small, in comparison with the pecuniary obstacles which had to be met during the whole of the period during which Colonel Warren was so bravely persevering in the recovery of monuments which have an undying interest for the whole Christian world.

2. The Geology of the Faröe Islands. By James Geikie, LL.D., F.R.SS. L. & E.

The author visited the Faröe Islands last summer in company with Mr Amund Helland of Christiania. They made various

traverses across the largest and most important islands, and touched here and there at several of the smaller ones. They have constructed a geological map of the group, upon which is shown the outcrop of the coal-seams of Suderöe, the direction of numerous dykes of basalt, the position of great intrusive sheets of the same rock; and the trend of the glaciation is indicated by arrows. The introductory part of this paper gives some account of the geological observations made by previous writers—Jorgen Landt in 1800, Mackenzie and Allan in 1815, Trevelyan a year or two later, Forchhammer in 1824, Robert Chambers in 1854, and Johnstrup in 1873. The general physical features of the islands are next described, the extent of land being roughly estimated at about 600 square miles. Nearly all the islands have an elongated form, and are drawn out in a N.N.W. and S.S.E. direction. This is likewise the direction of the more or less narrow sounds or open fiords that separate the islands in the northern part of the archipelago, as also of the wider belts of water in the south. All the islands have a mountainous character, and everywhere exhibit, in the most marked manner, the well-known terraced outline which is so common a feature of trappean masses, the highest elevation they attain is 2852 feet, but many of the hills approach to within 200 or 300 feet of that dominating point. The mean elevation of the northern group of islands is estimated to exceed 800 feet, and is probably not less than 900 feet. The coasts are usually precipitous, many of the cliffs exceeding 1000 feet, and in some places even 2000 feet in height. The valleys are described as ascending from the sea in a series of great steps or terraces—each terrace being cirque-shaped and framed in by a wall of rock, the upper surface of which stretches back to form the next cirque-like terrace, and so on in succession until the series abruptly terminates at the base, it may be, of some precipitous mountain. Occasionally the *col* between two valleys is so level that it is difficult to detect the actual water-parting. In this case the two valleys combine to form a kind of deep hollow passing right across the island from sea to sea. Lakes are very numerous, but of small size, and the streams are also abundant but of inconsiderable importance.

The author then goes on to describe the geological structure of the islands, which is extremely simple. The rocks consist principally of bedded basalts with intervening layers of tuff, and in Mygenæs

and Suderöe of clay, shale, and coal. The prevalent dip of the beds in the northern islands is south-easterly, but in Mygenæs it is easterly, and in Suderöe, or the southern island, north-easterly. Nowhere does the strata incline towards the west. The angles of dip are generally very low—in the northern islands not averaging more than 2° or 3°, while in Suderöe they are a degree or so higher.

The oldest part of the series is represented in Suderöe and Mygenæs. The basalt rocks in these islands consist principally of bedded anamesites, composed of plagioclase, augite, magnetite, and olivine. Their behaviour in the field and the general aspect they assume are described in considerable detail. They are all more or less amygdaloidal. Very often the various sheets of old lava are separated by partings and layers of a red fine-grained palagonitic tuff. Near the top of the anamesite series occurs an irregular belt or band of shales and clay with two seams of coal. [The position of this belt was shown upon a coloured diagram, representing a section across the island of Suderöe.] A thin and local seam of coal and shale is found much lower down in the series. [This also was shown upon the diagram.] The distance between this local coal and the workable coal-beds above is about 1100 feet. The author next gives a detailed description of the various outcrops of the coal, and traces its extension over Suderöe. [The more characteristic appearances presented by the coal were shown in another diagram.] The two workable coal-seams vary in thickness from a few inches up to two or three feet respectively. They are mined to a small extent, and in a very primitive manner.

The author next gives a particular account of the basalt rocks above the coal. It is these rocks which compose the major portion of the northern islands; the only one of the north islands which shows any trace of the coals and the lower igneous series being Mygenæs. The basalt rocks above the coals are for the most part more coarsely crystalline than those of the older series. They are dolerites rather than anamesites, but their composition is the same. They are also as a rule more coarsely amygdaloidal—the cavities often reaching a great size—two feet and even more in diameter. The minerals they contain are chiefly chabasite, stilbite, apophyllite, analcime, quartz, calcedony, calcspar, and green-earth, and it is not uncommon to find two, three, or even four different zeolites in one

and the same cavity. After describing in detail the various appearances assumed by the dolerites, and the features exhibited by the associated palagonitic tuffs, the author gives some account of the intrusive basalts, which are of two kinds—one occurring in sheets intruded along the line of bedding, the other in mere thin dykes and veins.

He estimates the thickness of the anamesites to be not less than 4000 feet, and that of the dolerites as between 9000 and 10,000 feet; thus giving a total thickness for the bedded volcanic rocks of not less than 13,000 or 14,000 feet. As the dip of the strata is extremely regular, and there are no large dislocations to complicate matters, this estimate may be relied upon as approximately correct.

He then enters into a lengthened discussion as to the origin of the strata, and combats the prevailing belief that the igneous rocks are relics of submarine eruptions. The conclusion come to is that the bedded basalt rocks of the Faröe Islands represent the heavy basic, and more liquid lavas which flowed from a cone or cones placed at some considerable distance, probably to the west of the present islands. The palagonitic tuffs, which sometimes contain small stones and grit, and are often laminated, represent partly fine volcanic dust (which the winds could carry considerable distances), partly volcanic mud, and to some extent they may also have been derived from the subaerial disintegration of the exposed lavas. They pass here and there into regular shales and tuffaceous clays, especially upon the horizon of the coal-seams. The coals are composed of land plants of Miocene age, and many plant remains occur in the associated clunch or clay and shale. None of these appear to have grown *in situ*—there are no roots penetrating an ancient soil. The coals are made up of the debris of plants carried down by freshets into shallow pools and marshy meres. Not a trace of marine organisms was observed in any strata throughout the islands. The coals and clays indicate a pause in the volcanic activity, during which the Miocene flora invaded the igneous area; but whether from the direction of America or Europe it is impossible to say. The local seam of coal at Dalbofos which occurs low down in the series, proves that there were more than one such pauses, and its fragmentary condition leads to the suspicion that the Miocene flora may have again and again invaded the

region, during long pauses between the eruptions;—all trace of these invasions having subsequently been destroyed by newer flows of lava.

The glacial phenomena are described in great detail under the following heads:—1. Glaciation; 2. Till or boulder-clay; 3. Erratics and Morainic debris; and 4. Rock-basins.

1. *Glaciation.*—Every island visited showed conspicuous marks of glacial abrasion; and notwithstanding that the rocks have suffered much since the glacial period from the action of the weather, striæ are yet well preserved in many places. They are very plentiful in and round Thorshavn, where they point E. 35° to 45° S. An examination of Stromöe and Osteröe, which were traversed in several places, and the smaller islands lying to the north-east, proved that the whole northern group had been buried under a thick sheet of ice, forming one compact *mer de glace* which flowed out in all directions from the dominant points of the islands. The glaciation was traced up to a height of 1600 feet, and as the water in some of the fiords is 100 fathoms deep, we must add this to the other measurement to get the maximum thickness of the ice (2200 feet), which flowed outwards from Faröe. The extreme northern ends of the islands were sought everywhere for indications of any invasion by ice from the direction of Greenland, but no trace of this was found; on the contrary, the rocks were there highly rubbed, polished, and striated in a direction from south to north. In Suderöe the glaciation goes up to 1400 feet; above that elevation the rocks are harsh, rugged, and serrated, just as they are above the limits of glaciation in the northern islands. The southern island showed that it too had been nearly smothered in ice, which moved off in all directions; and not only so, but it was evident from the direction and position of the striæ in the north of Suderöe that its *mer de glace* was coalescent with that of the northern islands. During the glacial period the Faröe Islands were thus united by one and the same ice-sheet, which had no connection with either the *mer de glace* of Greenland or that of Northern Europe. It was entirely a local ice-sheet flowing outwards from the main elevations of the islands, and breaking off all round, no doubt, in icebergs.

2. The Till is exactly comparable to the boulder-clay of Scot-

land, Scandinavia, and Switzerland. It is a more or less local accumulation of angular, subangular, and striated and smoothed stones and boulders, set in a matrix of hard gritty earth and clay. Its composition, its position with regard to the configuration of the ground,—sheltering as it does in the lee of rocks, whose polished faces look in the opposite direction,—and the mode of its distribution mark it out as the *moraine du fond* of the old *mer de glace.* Every stone it contained belonged to the islands, not a single fragment of any rock foreign to the Faröes occurring in it.

3. Erratics and Morainic debris are scattered about everywhere, and mark the retreat and gradual disappearance of the ice-sheet. Many of these erratics attain a large size, some measuring upwards of 20 feet across. Not one of them is foreign to Faröe. Few well-marked terminal moraines in the valleys were observed, partly owing to the fact that great quantities of debris have fallen from the cliffs, and tended to obscure the glacial debris heaps, and partly because they have suffered much from the action of torrents and freshets. Here and there, however, mounds of morainic origin are conspicuous enough.

4. The Rock-basins are next described, and their origin assigned to the grinding action of the glaciers. They are numerous upon the land, and seem to be as common a feature of the fiords of Faröe as they are of the Scottish and Norwegian sea-lochs.

The latter part of the paper discusses the origin of the valleys and fiords, which is ascribed partly to subaerial erosion and partly to glacial excavation. The origin of the main water-parting of the islands also comes in for discussion, and considerable space is devoted to the consideration of such topics as present atmospheric and marine erosion in the islands. The paper concludes with some account of the peat with its remains of small trees, and a description of a number of typical rock specimens.

3. By special permission of the Society, there was read a Meteorological Note by Mr Alexander Wallace. Communicated by Professor Piazzi Smyth.

On Monday, March 1, 1880, a strong breeze from the S.W., marked at 1 o'clock P.M. in meteorological observation, as equal to 15 miles

per hour, prevailed, after which the force of the wind rapidly increased, shifting to W.N.W. until about 2 o'clock when it became a perfect hurricane, accompanied by a storm of snow and sleet. The velocity of the wind must have been betwixt 40 and 50 miles per hour, and partly from heavy black clouds covering the sky, and partly from the dense sheet of snow which was drifting along, everything became obscured.

From the point where I stood (at the door of my house, the Old Observatory, Calton Hill) there appeared two currents of snow-drift, one on each side, which meeting each other about 20 feet in front of me, after a severe struggle coalesced and shot upwards with excessive velocity slantingly, and towards E.S.E. As that upshot was going or gone, but when the storm was still at its maximum, a loud crash was heard, and a vivid flash of light simultaneously seen, much as one may suppose would be the effect of the bursting of a bomb-shell within a few feet of you.

Almost immediately after this the storm began to abate, and the remainder of the day was comparatively calm. Altogether the storm did not last above ten minutes, and during that time the barometer fell $\frac{2}{10}$ths of an inch, but rose again immediately afterwards to its former height.

4. On the Colouring of Maps. By Professor Tait.

(*Abstract.*)

Some years ago, while I was still working at knots, Professor Cayley told me of De Morgan's statement that four colours had been found by experience to be sufficient for the purpose of completely distinguishing from one another the various districts on a map.

I had previously shown that if an even number of boundaries meet at each point on a diagram, two colours (as on a chess-board) will suffice for the purpose. But in a map, boundaries usually meet in threes.

I replied to Professor Cayley that I thought the proof might be made to depend upon the obvious proposition that not more than four points in a plane can be joined two and two by non-intersecting lines. Here points were made to stand for districts. When two such points are joined by a line they must have different colour-titles. I

did not at the time pursue the subject, as I found that it was more complex than it appeared at first.

Mr Kempe's paper in *Nature* (February 26, 1880) has recalled my attention to the subject, and some simple modes of treating the question have occurred to me. The germs of them are in what I have said above, and they show one easily how to proceed to colour any map. A sketch only of one of them is now given.

Begin by making as above stated a companion diagram, putting points for districts, and lines joining them for common boundaries. Then by introducing (in any way) as many new joining lines as possible (but so that no two intersect) the diagram is divided into three-sided compartments.

Next, make all of these compartments four-sided by taking a number of new points, each on a joining line. The whole set of points can now be lettered A and B alternately, because two colours suffice for a map whose boundaries meet in fours. But let the intruded points be lettered a and b, instead of A and B respectively.

Now perform the same operation in a second way, differing everywhere from the first, and call the newly intruded points α and β instead of A and B.

Rules are laid down for carrying out these operations; but they require too many illustrative cuts to be given here.

Then any one triangular compartment will appear in two essentially different forms: for instance, with its intruded points it may read (in the two cases, taking the corners in the same order) B, a, B, A and B, A, β, A. Now superpose the two figures, lettering included, and attend to the order of the two letters at the same point. We have, from the instance above, the compound reading (attending now to the corners only) BB, BA, AA, of which the separate terms are necessarily different. Hence every point in the figure is lettered differently from all that are joined to it, and only four designations can occur, viz.: AA, AB, BA, BB. This proves the proposition, and gives one mode of colouring the original map. For the *erasure* of joining lines (such as were originally introduced to divide the whole into three-sided compartments) does not necessitate any change of lettering.

This mode of treating the question shows incidentally that in a map where only three boundaries meet at each point, the boundaries

may be coloured with *three* colours, so that no two of the same colour are conterminous.

This particular process essentially introduces four different colours, and therefore does not necessarily give the simplest way of colouring a map. Another method, quite different from this, but involving virtually the same principles, is next given. Then come two other processes, different in form from that of Mr Kempe, but based like it ultimately on the fact that only $3(n-2)$ non-intersecting lines can be drawn (except for $n=2$) joining n points in a plane.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. X. 1879–80. No. 107.

NINETY-SEVENTH SESSION.

Monday, 5th April 1880.

SIR WYVILLE THOMSON, Vice-President, in the Chair.

The following Communication was read:—

1. On the Structure and Origin of Coral Reefs and Islands. By John Murray.

(*Abstract.*)

Darwin's Theory.—During the voyage of the "Beagle" and subsequently, Mr Darwin made a profound study of coral reefs, and has given a theory of their mode of formation which has since been universally accepted by scientific men.

Darwin's theory may be said to rest on two facts—the one physiological, and the other physical—the former, that those species of corals whose skeletons chiefly make up reefs cannot live in depths greater than from 20 to 30 fathoms; the latter, that the surface of the earth is continually undergoing slow elevation or subsidence.

The corals commence by growing up from the shallow waters surrounding an island, and form a fringing reef which is closely attached to the shore. The island slowly sinks, but the corals continually grow upwards, and keep the upper surface of the reef at a level with the waves of the ocean. When this has gone on for some time a wide navigable water channel is formed between the

reef and the shores of the island, and we have a barrier reef. These processes have but to be continued some stages further, when the island will disappear beneath the ocean, and be replaced by an atoll with its lagoon where the island once stood.

According to this simple and beautiful theory, the fringing reef becomes a barrier reef, and the barrier reef an atoll by a continuous process of development.

Object of the Present Paper.—Professor Semper,* during his examination of the coral reefs in the Pelew group, experienced great difficulties in applying Darwin's theory. Similar difficulties presented themselves to the author in those coral reef regions visited during the cruise of the "Challenger."

The object of the present paper is to show, *first*, that, while it must be granted as generally true that reef-forming species of coral do not live at a depth greater than 30 or 40 fathoms, yet that there are other agencies at work in the tropical oceanic regions by which submarine elevations can be built up from very great depths so as to form a foundation for coral reefs; *second*, that while it must be granted that the surface of the earth has undergone many oscillations in recent geological times, yet that all the chief features of coral reefs and islands can be accounted for without calling in the aid of great and general subsidences.

Nature of Oceanic Islands and Submarine Elevations.—It is now known that, with scarcely an exception,† all oceanic islands other than coral atolls are of volcanic origin. Darwin, Dana, and others have noticed the close resemblance between atolls and ordinary islands in their manner of grouping as well as in their shapes. In a previous paper the author pointed out the wide distribution of volcanic debris over the bed of the ocean in tropical regions, and the almost total absence of minerals, such as quartz, which are characteristic of continental land.‡ There is every reason for believing that atolls are primarily situated on volcanic mountains and not on submerged continental land as is so often supposed.

* Zeitschr. für Wissen. Zoologie, vol. xiii. p. 563.

† New Zealand, New Caledonia, and the Seychelles have primitive rocks, if these can be regarded as oceanic islands. Some of the islands between New Caledonia and Australia may have primitive rocks, and the atolls in these regions may be situated on foundations of this nature.

‡ Proc. Roy. Soc. Edin., 1876–77, p. 247.

The soundings of the "Tuscorora" and "Challenger" have made known numerous submarine elevations: mountains rising from the general level of the ocean's bed, at a depth of 2500 or 3000 fathoms, up to within a few hundred fathoms of the surface. Although now capped and flanked by deposits of Globigerina and Pteropod ooze, these mountains were most probably originally formed by volcanic eruptions. The deposits in deep water on either side of them were almost wholly made up of volcanic materials.

Volcanic mountains situated in the ocean basins, and which during their formation had risen above the surface of the water, would assume a more or less sharp and pointed outline owing to the denuding action of the atmosphere and of the waves, and very extensive banks of the denuded materials would be formed around them. Some, like Graham's Island, might be wholly swept away, and only a bank with a few fathoms of water over it be left on the spot. In this way numerous foundations may have been prepared for barrier reefs and even atolls.

Those volcanoes which during their formation had not risen above the surface of the sea (and they were probably the most numerous) would assume a rounded and dome-like contour,* owing to the denser medium into which the eruptions had taken place, and the deposits which had been subsequently formed on their summits.

In order to clearly understand how a submarine mountain, say half a mile beneath the sea, can be built up sufficiently near the surface to form a foundation on which reef-forming corals might live, it is necessary to consider attentively the

Pelagic Fauna and Flora of Tropical Regions.—During the cruise of the "Challenger," much attention was paid to this subject. Every day while at sea tow-nets were dragged through the surface waters; and while dredging they were sent down to various depths beneath the surface. Everywhere life was most abundant in the surface and sub-surface waters. Almost every haul gave many calcareous, siliceous, and other Algæ; great numbers of Foraminifera and Radiolaria, Infusoria, Oceanic Hydrozoa, Medusæ, Annelids; vast numbers of microscopic and other Crustacea, Tunicates, Pelagic Gastropods, Pteropods, Heteropods, Cephalopods,

* Scrope on Volcanoes, chap. viii.

Fishes, and fish-eggs; larvæ of Echinoderms, and of many of the above creatures, &c.

Most of these organisms live from the surface down to about 100 fathoms.* In calm weather they swarm near the surface, but when it is rough they are to be found several fathoms beneath the waves. They are borne along in the great oceanic currents which are created by the winds; and meeting with coral reefs, they supply the corals on the outer edge of the reefs with abundant food. The reason why the windward side of a reef grows more vigorously appears to be this abundant supply of food, and not the more abundant supply of oxygen as is generally stated. The "Challenger" researches showed that oxygen was particularly abundant in all depths inhabited by reef-forming corals.

When these surface animals die, either by coming in contact with colder water or from other causes, their shells and skeletons fall to the bottom, and carry down with them some organic matter which gives a supply of food to deep-sea animals. The majority of deep-sea animals live by eating the mud at the bottom.

An attempt was made to estimate the quantity of carbonate of lime, in the form of calcareous Algæ, Foraminifera, Pteropods, Heteropods, Pelagic Gastropods, in the surface waters. A tow-net, having a mouth $12\frac{1}{2}$ inches in diameter, was dragged for as nearly as possible half a mile through the water. The shells collected were boiled in caustic potash, washed, and then weighed. The mean of four experiments gave 2·545 grammes. If these animals were as abundant in all the depth down to 100 fathoms as they were in the track followed by the tow-net, this would give over 16 tons of carbonate of lime in this form in a mass of the ocean one mile square by 100 fathoms.†

* The Challengeridæ, and many of the other members of Haeckel's new order *Phæodaria*, certainly live deeper, as we never got them in the tropics except when the net was sent down to a depth of 200 or 300 fathoms.

† Among the varieties of Foraminifera recognised by Mr Brady in the "Challenger" collections, the following have a Pelagic habitat:—

Pulvinulina Menardii.	*Pullenia obliquiloculata.*
,, *canariensis.*	*Sphærodina dehiscens.*
,, *crassa.*	*Caudeina nitida.*
,, *Micheliniana.*	*Hastigerina Murrayi.*
,, *tumida.*	,, *pelagica.*

Bathymetrical Distribution of the Calcareous Shells and Skeletons of Surface Organisms.—Although these lime-secreting organisms are so abundant in tropical surface waters, their cast-off shells and skeletons are either wholly or partially absent from by far the greater part of the floor of the ocean. In depths greater than 3000 fathoms we usually met with only a few shells of Pelagic Foraminifera of the larger and heavier kinds; a few hundred fathoms nearer the surface they became more numerous, and we get a few of the smaller kinds and some Coccoliths and Rhabdoliths. At about 1900 or 1800 fathoms a few shells of Pteropods and Heteropods are met with; and in all depths less than a mile we have a deposit in which the shell and skeletons of almost every surface organism is to be found. In the equatorial streams and calms the calcareous Algæ, Pelagic Foraminifera, Pteropods, and Heteropods are more abundant on the surface than elsewhere; and it is in these same regions that we found their dead shells at greater depths than in

Orbulina universa.	*Globigerina dubia.*
Globigerina bulloides.	,, *rubra.*
,, *æquilateralis.*	,, *conglobata*
,, *sacculifera* (hirsuta).	,, *inflata.*

It is the dead shells of these Pelagic Foraminifera which chiefly make up the calcareous oozes of the deep sea. The living shells of all the above varieties swarm in the tropical and sub-tropical waters near the surface. It is especially in the region of the equatorial calms that the largest and thickest shelled specimens are found. As we go north or south into colder water they become smaller, and many varieties die out. In the surface waters of the Arctic and Antarctic regions, only some dwarfed specimens of *Globigerina bulloides* are met with. The author is unable to agree with Dr Carpenter and Mr Brady in thinking that these Pelagic Foraminifera also live on the bottom. This question was made the subject of careful investigation during the cruise. The shells from the surface and from the bottom were compared at each locality, and it was found, by micrometric measurement, that surface specimens were as large and as thick shelled as any average specimens from the soundings. It is quite unlikely that the same individuals should pass a part of their lives in the warm sunny surface waters, at a temperature of from 70° to 80° Fahr., and another part in the cold dark waters two or three miles beneath, at a temperature of 30° or 40° Fahr. The geographical distribution of these Pelagic forms over the bottom coincides exactly with the distribution of the same forms on the surface; that is to say, both on the surface and on the bottom, the distribution is ruled by surface temperature. No specimens of these Pelagic varieties were ever obtained from the bottom with the shells filled and surrounded with sarcode. Whereas creeping and attached forms (like Truncatulina, Discorbina, Anomalina, and some Textulariæ) were taken in this condition in almost every dredge. These last-mentioned forms which we know live on the bottom have a distribution quite independent of surface temperature.

the deposits of other parts of the ocean. Another circumstance influences the bathymetrical distribution of these surface shells. When there is a complete and free oceanic circulation from the top to the bottom, these dead shells are found at greater depths in the deposits than where the circulation is cut off by submarine barriers.

The agent by which these shells are removed is, as Sir Wyville Thomson suggested, carbonic acid. Analysis shows that carbonic acid is most abundant in sea water, and especially so in deep water. Pteropod and Heteropod shells are very much larger than the Foraminifera, yet are very much thinner; and hence, for the quantity of lime contained in them, they present a much greater surface to the action of the sea water. This seems to be the reason why all large and thin shells are first removed from the deposits with increasing depth, and not the fact that some shells are composed of arragonite and some of calcite, as has been suggested.

There is a continual struggle in the ocean with respect to the carbonate of lime. Life is continually secreting it and moulding it into many varied and beautiful forms. The carbonic acid of ocean waters attacks these when life has lost its hold, reduces the lime to the form of a bicarbonate, and carries it away in solution. In all the greater depths of the ocean these surface shells are reduced to a bicarbonate either during their fall through the water or shortly after reaching the bottom.

In the shallower depths—on the tops of submarine elevations or volcanoes—the accumulation of the dead silicious and calcareous shells is too rapid for the action of the sea water to have much effect. Long before such a deposit reaches sufficiently near the surface to serve as a foundation for reef-forming corals, it is a bank on which flourish numerous species of Foraminifera, Sponges, Hydroids, deep-sea Corals, Annelids, Alcyonarians, Molluscs, Polyzoa, Echinoderms, &c. All these tend to fix and consolidate such a bank, and add their shells, spicules, and skeletons to the relatively rapid accumulating deposits. Eventually coral-forming species attach themselves to such banks, and then commences the formation of

Coral Atolls—Mr Darwin has pointed out that "reefs not to be distinguished from an atoll might be formed" * on submerged banks such as those here described. However, the improbability of

* Coral Reefs, p. 118.

so many submerged banks existing in the open ocean caused him to reject this mode of formation for atolls. As here stated, recent deep-sea investigations have shown that submerged banks are continually in process of formation in the tropical regions of the ocean, and it is in a high degree probable that the majority of atolls are seated on banks formed in this manner.

Mr Darwin has also pointed out that the corals on the outer margin of a submerged bank would grow vigorously, whilst the growth of those on the central expanse would be checked by the sediment formed there, and by the small amount of food brought to them.* Very early in the history of such an atoll, and while yet several fathoms submerged, the corals situated on the central parts would be placed at a disadvantage, and this would become greater and greater as the coral plantations approached the surface. When the coral plantation was small there was a relatively large periphery for the supply of food to the inner parts, and also for the supply of sediment; and hence, in small atolls the lagoon was very shallow, and was soon filled up. For the same reasons coral islands situated on long and narrow banks have no lagoons. An atoll one mile square has a periphery of four miles. In an atoll four miles square —the periphery increasing in arithmetical progression and the area as the square—we have for each square mile only a periphery of one mile over which food may pass to the interior, and from which sediment is supplied for filling up the lagoon.

With increasing size, then, the conditions become more and more favourable to the formation of lagoons, and as a consequence we have no large or moderate sized coral islands without lagoons. Townet experiments always showed very much less Pelagic life (food) in the lagoon waters than on the outer edge of the reef. The lagoon becomes less favourable for the growth of all the more massive kinds of coral as the outer edge of the reef reaches the surface, and cuts off the free supply of ocean waters. Many species of corals die.† Much dead coral, coral rock, and sediment is exposed to the solvent action of the sea water. Larger quantities of lime are carried away in solution as a bicarbonate from the lagoon than are

* Coral Reefs, p. 134.

† There are no living corals or shells in some small lagoons, the waters of which become highly heated, and in some cases extremely saline.

secreted by the animals which can still live in it; the lagoon thus becomes widened and deepened.*

On the other hand a vigorous growth and secretion of lime takes place on the outer margins of the reef; and when the water outside becomes too deep for reef-forming corals to live, these still build seawards on a talus made up of their own debris:—the whole atoll expands somewhat after the manner of a Fairy Ring.

It is not necessary to call in disseverment of large atolls in order to explain the appearances presented in the Great Maldiva group of atolls.† The coral fields rising from very many parts of these extensive submarine banks form atolls. The marginal atolls have from the first the advantage of a better supply of food. They elongate in the direction of the margin of the bank where the water is shallower than to seaward. Many of these marginal atolls have coalesced, and as this growth and coalescence have continued, a large part of the food-supply has been cut off from the small atolls situated towards the interior of the bank. Ultimately a large atoll like Suadiva atoll would be formed. The atolls in the interior would be perhaps wholly removed in solution, and the atoll-like character of small marginal but now coalesced atolls would be wholly or partially lost by the destruction of their inner sides.‡ A study of the charts shows all the stages in this mode of development.

In the case of the Lakadivh, Caroline, and Chagos archipelagos we have submarine banks at various stages of growth towards the surface, some too deep for reef-forming species of coral, others with coral plantations, but all submerged several fathoms, and scattered amongst these some of the oldest and most completely-formed atolls and coral islands. It is most difficult to conceive how these sub-

* Complete little Serpula-atolls, with lagoons from 3 to 50 feet in diameter, and formed in this way without subsidence, were numerous along the shores of Bermuda.

† Mr Darwin's application of his theory to this group—where the disseverment of large atolls is called in, and a destructive power attributed to oceanic currents, which it is very unlikely they can ever possess—has often been considered unsatisfactory.

‡ "In speaking of Bow Island, Belcher mentions the fact that several of its points had undergone material change, or were no longer the same when visited after the lapse of fourteen years. These remarks refer particularly to islets situated within the lagoon. I could myself quote many instances of the same description."—"Wilkes' Exploring Expedition," vol. iv. p. 271.

merged banks could have been produced by subsidence, situated as they are in relation to each other and with respect to the perfectly-formed atolls of the groups.

It is a much more natural view to regard these atolls and submerged banks as originally volcanoes reaching to various heights beneath the sea, and which have subsequently been built up to and towards the surface by accumulations of organic sediment and the growth of coral on their summits. It is a remarkable fact that, in all coral atolls which have been raised several hundred feet above the sea, the base is generally described as composed of solid limestone, or "of various kinds of coral evidently deposited after life had become extinct." * This base is probably often made up of such a rock as that brought by the missionaries from New Ireland, and described by Professor Liversidge,† as composed chiefly of Pelagic Foraminifera, the same as those taken by the "Challenger" in the surface waters of the Pacific.

Microscopic sections of a rock taken from 50 feet below sea level at Bermuda show that a deposition of carbonate of lime is going on. The small shells are filled with, and the broken pieces of shells and corals are cemented by, calcite. The wells in coral islands rise and fall with the tide, so that the whole atoll is filled like a sponge with sea water. This water is very slowly interchanged, and by the solution of the smaller and thinner particles, becomes saturated, and a deposition of lime follows. In this way we may explain the absence of many of the more delicate shells from some limestones.‡

Barrier Reefs.—During the visit of the "Challenger" to Tahiti, a careful examination was made of the reefs by dredging, sounding, &c., in a steam pinnace, both inside and outside the reefs. Lieutenant Swire of the "Challenger" made a careful trigonometrical survey of the profile of the outer reefs on six different lines; and while associated with him in this work, the author was indebted to that officer for many valuable suggestions.

A ledge ran out from the edge of the reef to about 250 yards, where we got a depth of from 30 to 40 fathoms. It was covered with a most luxuriant growth of coral bosses and knobs.

* U. S. Ex. Exp., vol. iv. p. 269. † Geol. Mag., Dec. 1877.

‡ Fuchs, Über die Entstehung der Aptychenkalke. Sitzb. der k. Akad. der Wissensch. 1877.

Between 250 and 350 yards from the edge of the reef there was generally a very steep and irregular slope; about 100 fathoms was got at the latter distance, and the angles between these last-mentioned distances often exceeded 45 degrees. The talus here appeared to be composed of huge masses and heads of coral, which had been torn by the waves from the upper ledge and piled up on each other. They were now covered with living Sponges, Alcyonarians, Hydroids, Polyzoa, Foraminifera, &c.*

From 350 to 500 yards from the edge of the reef, we had a slope with an angle of about 30°, and made up chiefly of coral sand. Beyond 500 yards the angle of the slope decreased till we had at a distance of a mile from the reef an angle of 6°, a depth of 590 fathoms, and a mud composed of volcanic and coral sand, Pteropods, Pelagic and other Foraminifera, Coccoliths, &c.

In the lagoon channel the reefs were found to be fringed with living coral, and to slope downwards and outwards for a few feet, and then plunge at once to a depth of 10 or 16 fathoms. Many portions of these inner reefs were overhanging, and at some places overhanging masses had recently fallen away. Everywhere much dead coral rock was exposed to the solvent action of the sea water. The reefs of Tahiti are at some places fringing, at other places there is a boat passage within the reef, and at Papiete there is a large ship channel with islets within, and the outer edge of the reef is a mile distant from the shore. The island itself is surrounded with a belt of fertile low land, frequently three or four miles wide; this shows that the island has not in recent times undergone subsidence; there are, indeed, reasons for supposing it has recently been slightly elevated. Everything appears to show

* This ledge and steep slope beyond where a depth of 30 or 40 fathoms was reached, was characteristic of a large number of atoll and barrier reefs, and seemed due to wave action. Experiments had been made with masses of broken coral, and it was found that these could (on account of their rough and jagged surface) be built up into a nearly perpendicular wall by letting them fall on each other. A talus formed in water deeper than 40 fathoms where there was little if any motion would be different from one formed on land. In the latter case the disintegrating forces at work always tended to set the talus in motion; in the former case everything tended to consolidate and to fix the blocks in the positions first assumed. A removal of lime in solution would take place from the blocks forming this steep slope, but, except in very deep water, this would not be sufficient to check the outward extension of the reef.

that the reefs have commenced close to the shore and have extended seawards, first on a foundation composed of the volcanic detritus of the island, and afterwards on a talus composed of coral debris, and the shells and skeletons of surface organisms.*

The lagoon channel was subsequently slowly formed by the solvent action of the sea water thrown over the reefs at each tide, and the islets in the lagoon channel are portions of the original reef still left standing. The reefs have extended outwards from the island and have been disintegrated and removed behind in the same way as the atoll has extended outwards after reaching the surface.

Where reefs rise quite to the surface, and are nearly continuous, we find relatively few coral patches and heads in the lagoons and lagoon channels. Where the outer reefs are much broken up, the coral growths in the lagoon are relatively abundant. Where the water was deep and the talus to be formed was great, the outward growth has been relatively slow,† and the disintegrating forces in the lagoons and lagoon channels gaining in the struggle, the reefs would become very narrow and might indeed be broken up. This, however, would admit the oceanic waters and more food, and growth would again commence on the inner as well as the outer sides of the still remaining portions. In the great barrier reef of Australia, where the openings are numerous and wide, the reefs have a great width. Where the openings are few and neither wide nor deep (as in lat. 12° 30′) the reefs are very narrow and "steep to"—on their inner side.

At the Admiralty Islands, on the lagoon side of the islets on the barrier reefs, the trees were found overhanging the water, and in some cases the soil washed away from their roots. It is a common observation in atolls that the islets on the reefs are situated close to the lagoon shore. These facts point out the removal of matter which is going on in the lagoons and lagoon channels.

Elevation and Subsidence.—Mr Darwin has given many reasons for believing that those islands and coasts which have fringing reefs had recently been elevated, or had long remained in a state of rest.

* A dredging in 155 fathoms, close to the barrier reef of Australia (between it and Raine Island), gave a coral sand, which was, I estimate, more than two-thirds made up of the shells of surface animals.

† Hence in barrier reefs, where the depth outside is very great, we find the reefs running closer to the shore than where the depth is less, and consequently the talus to be formed is smaller.

Throughout the volcanic islands of the great ocean basins the evidence of recent elevations are everywhere conspicuous. Jukes has given most excellent reasons for believing that the coast of Australia fronted by the barrier reef, and even the barrier reef itself, have recently been elevated.* Dana and Couthouy have given a list of islands in almost every barrier reef and atoll region which have recently been elevated.†

This is what we should expect. Generally speaking, all the volcanic regions which we know have in the main been areas of elevation, and we would expect the same to hold good in those vast and permanent hollows of the earth which are occupied by the waters of the ocean. It must be remembered that, probably, all atolls were seated on submarine volcanoes. Areas of local depression are to be looked for in the ocean basins on either side of and between groups of volcanic islands and atolls, and not on the very site of these islands. This is what the deep-sea soundings show if they show any depression at all. Subsidence has been called in in order to account for the existence of lagoons and lagoon channels, and the narrow bands of reef which enclose these; but it has been shown that these were produced by quite other causes,—by the vigorous growth of the corals where most nourishment was to be had, and their death solution and disintegration by the action of sea-water and currents‡ at those parts which cannot be, on account of their situation, sufficiently supplied with food.

All the chief and characteristic features of barrier reefs and atolls may, indeed, exist with slow elevation, for the removal of lime from the lagoons and the dead upper surface of the reefs by currents, and in solution by rain and sea-water might keep pace with the upward movement.

The most recent charts of all coral reef regions have been examined, and it is found possible to explain all the phenomena by the principles here advanced; while on the subsidence theory, it is most difficult to explain the appearances and structures met with in

* Voyage of the Fly, vol. i. p. 335.

† Dana's Corals and Coral Islands, p. 345. Couthouy's "Remarks on Coral Formations," Bost. Jour. Nat. Hist. See also Stutchbury, West of England Journal.

‡ Very strong currents run out of the entrances into lagoons and lagoon channels, and when the tow-net was used in these entrances it showed that a large quantity of coral detritus was being carried seawards.

many groups; for instance in the Fiji Islands, where fringing reefs, barrier reefs, and atolls, all occur in close proximity, and where all the other evidence seems to point to elevation, or at least a long period of rest. In instances like the Gambier group, the reefs situated on the seaward side of the outer islands would grow more vigorously than those towards the interior; they would extend in the direction of the shallower water, and ultimately would form a continuous barrier around the whole group. The distinguishing feature of the views now advanced is that they do away with the great and general subsidences required by Darwin's theory,* and are in harmony with Dana's views of the great antiquity and permanence of the great ocean basin, which all recent deep-sea researches appear to support.

Summary.—It was shown (1) that foundations have been prepared for barrier reefs and atolls by the disintegration of volcanic islands, and by the building up of submarine volcanoes by the deposition on their summits of organic and other sediments.

(2.) That the chief food of the corals consists of the abundant Pelagic life of the tropical regions, and the extensive solvent action of sea-water is shown by the removal of the carbonate of lime-shells of these surface organisms from all the greater depths of the ocean.

(3.) That when coral plantations build up from submarine banks they assume an atoll form, owing to the more abundant supply of food to the outer margins, and the removal of dead coral rock from the interior portions by currents and by the action of the carbonic acid dissolved in sea-water.

(4.) That barrier reefs have built out from the shore on a foundation of volcanic debris or on a talus of coral blocks, coral sediment, and Pelagic shells, and the lagoon channel is formed in the same way as a lagoon.

(5.) That it is not necessary to call in subsidence to explain any of the characteristic features of barrier reefs or atolls, and that all these features would exist alike in areas of slow elevation, of rest, or of slow subsidence.

In conclusion it was pointed out that all the causes here appealed

* "We may conclude that immense areas have subsided, to an amount sufficient to bury not only any formerly existing lofty table-land, but even the heights formed by fractured strata and erupted matter."—"Coral Reefs," p. 190.

to for an explanation of the structure of coral reefs are proximate, relatively well known, and continuous in their action.

The author expressed his indebtedness to all his colleagues, to Professor Geikie, to the Hydrographer and officers of the hydrographic department, and in a special manner to Sir Wyville Thomson, under whose direction and advice all the observations had been conducted.

BUSINESS.

The following candidates were balloted for, and declared duly elected Fellows of the Society:—Major-General Bayly, R.E.; Mr W. J. Sollas, M.A.; and Mr Henry Drummond, F.G.S.

Monday, 19th April 1880.

SIR WYVILLE THOMSON, Vice-President, in the Chair.

The following Communications were read:—

1. Rock-Weathering, as illustrated in Edinburgh Churchyards. By Professor Geikie, F.R.S. (Plate XVI.)

Comparatively little has yet been done in the way of precise measurement of the rate at which the exposed surfaces of different kinds of rock are removed in the processes of weathering. A few years ago, some experiments were instituted by Professor Pfaff of Erlangen to obtain more definite information on this subject. He exposed to ordinary atmospheric influences carefully measured and weighed pieces of Solenhofen limestone, syenite, granite (both rough and polished), and bone. At the end of three years he found that the loss from the limestone was equivalent to the removal of a uniform layer 0·04 mm. in thickness from its general surface. The stone had become quite dull and earthy, while on parts of its surface fine cracks and incipient exfoliation had appeared.* The time during which the observations were continued was, however, too brief to allow any general deductions to be drawn from them as to the real average rate of disintegration. Professor Pfaff relates that during the period a severe hail storm broke one of the plates of stone. An exceptionally powerful cause of this nature might make

* Allgemeine Geologie als exacte Wissenschaft, p. 317.

the loss during a short interval considerably greater than the true average of a longer period.

It occurred to me recently that data of at least a provisional value might be obtained from an examination of tombstones freely exposed to the air in graveyards, in cases where their dates remained still legible or might be otherwise ascertained. I have accordingly paid attention to the older burial-grounds in Edinburgh, and have gathered together some facts which have, perhaps, sufficient interest and novelty to be communicated to the Society.

At the outset it is of course obvious that in seeking for data bearing on the general question of rock-weathering, we must admit the kind and amount of such weathering visible in a town to be in some measure different from what is normal in nature. So far as the disintegration of rock-surfaces is effected by mineral acids, for example, there must be a good deal more of such chemical change where sulphuric acid is copiously evolved into the atmosphere from thousands of chimneys than in the pure air of country districts. In these respects we may regard the disintegration in towns as an exaggeration of the normal rate. Still, the difference between town and country may be less than might be supposed. Surfaces of stone are apt to get begrimed with dust and smoke, and the crust of organic and inorganic matter deposited upon them may in no small measure protect them from the greater chemical activity of the more acid town rain. In regard to the effect of daily or seasonal changes of temperature, on the other hand, any difference between town and country may not impossibly be on the side of the town. Owing, probably, to the influence of smoke in retarding radiation, thermometers placed in open spaces in town commonly mark an extreme nocturnal temperature not quite so low as those similarly placed in the suburbs, while they show a maximum day temperature not quite so high.

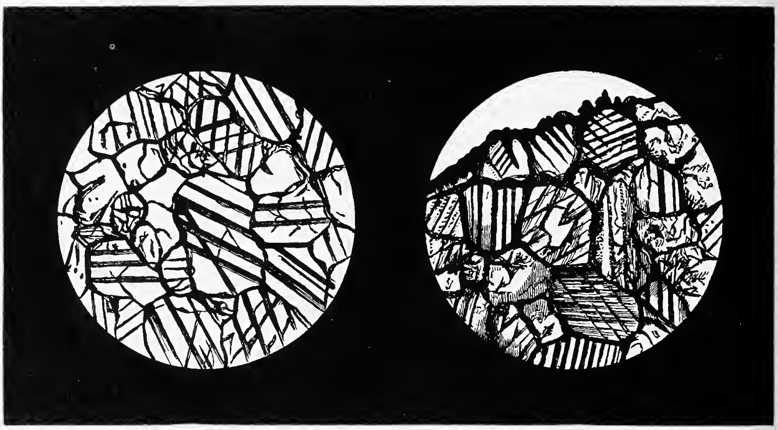
The illustrations of rock-weathering presented by city graveyards are necessarily limited to the few kinds of rock employed for monumental purposes. In this district the materials used are of three kinds:—1st, Calcareous, including marbles and limestones; 2d, sandstones and flagstones; 3d, granites.

I. Calcareous.—With extremely rare exceptions, the calcareous tombstones in our graveyards are constructed of ordinary white

saccharoid Italian marble. I have also observed a pink Italian shell-marble, and a finely fossiliferous limestone, containing fragments of shells, foraminifera, &c.

In a few cases the white marble has been employed by itself as a monolith in the shape of an obelisk, urn, or other device; but most commonly it occurs in slabs which have been tightly fixed in a framework of sandstone. These slabs, from less than 1 to fully 2 inches thick, are generally placed vertically; in one or two examples they have been inserted in large horizontal sandstone slabs or "through-stanes." The form into which it has been cut, and the position in which it has been erected, have had considerable influence on the weathering of the stone.

A specimen of the common white marble employed for monumental purposes was obtained from one of the marble-works of the city, and examined microscopically. It presented the well-known granular character of true saccharoid marble, consisting of rounded granules of clear transparent calcite, averaging about $\frac{1}{100}$th of an inch in diameter (fig. 1, A). Each granule has its own system of



A B

Fig. 1.—Microscopic structure of white marble employed in Edinburgh tombstones. A, Structure of the fresh marble. B, Structure of the marble after standing eighty-seven years. The black edge is the crust of sulphate of lime and town dust which descends along rifts and cleavage planes.

twin lamellations, and not infrequently gives interference colours. The fundamental rhombohedral cleavage is everywhere well de-

veloped. Not a trace exists of any amorphous granular matrix or base holding the crystalline grains together. These seem moulded into each other, but have evidently no extraordinary cohesion. A small fragment placed in dilute acid was entirely dissolved. There can be no doubt that this marble must be very nearly pure carbonate of lime.

The process of weathering in the case of this white marble presents three phases sometimes to be observed on the same slab,—viz., superficial solution, internal disintegration, and curvature with fracture.

(1.) *Superficial Solution* is effected by the carbonic acid, and partly by the sulphuric acid of town-rain. When the marble is first erected it possesses a well-polished surface, capable of affording a distinct reflection of objects placed in front of it. Exposure for not more than a year or two to our prevalent westerly rains suffices to remove this polish, and to give the surface a rough granular character. The granules which have been cut across or bruised in the cutting and polishing process are first attacked and removed in solution, or drop out of the stone. An obelisk in Greyfriars' Churchyard, erected in memory of a lady who died in 1864, has so rough and granular a surface that it might readily be taken for a sandstone. So loosely are the grains held together that a slight motion of the finger will rub them off. In the course of solution and removal, the internal structure of the marble begins to reveal itself. Its harder nests and veinings of calcite and other minerals project above the surrounding surface, and may be traced as prominent ribs and excrescences running across the faint or illegible inscriptions. On the other hand, some portions of the marble are more rapidly removed than others. Irregular channels, dependent partly on the direction given to trickling rain by the form of the monumental carving, but chiefly on original differences in the internal structure of the stone, are gradually hollowed out. In this way the former artificial surface of the marble disappears, and is changed into one that rather recalls the bare bleached rocks of some mountain side.

The rate at which the transformation takes place seems to depend primarily on the extent to which the marble is exposed to rain. Slabs which have been placed facing to north-east, and with a sufficiently projecting architrave to keep off much of the rainfall, retain

their inscriptions legible for a century or longer. But even in these cases the progress of internal disintegration is distinctly visible. Where the marble has been less screened from rain, the rapidity of waste has been sometimes very marked. A good illustration is supplied by the tablet on the south side of Greyfriars' Churchyard, erected in memory of G—— G——, who died in 1785.* This monument had become so far decayed as to require restoration in 1803. It is now and has been for some years for the most part utterly illegible. The marble has been dissolved away over the centre of the slab to a depth of about a quarter of an inch. Yet this monument is by no means in an exposed situation. It faces eastward in a rather sheltered corner, where, however, the wind eddies in such a way as to throw the rain against the part of the stone which has been most corroded.

In the majority of cases superficial solution has been retarded by the formation of a peculiar grey or begrimed crust, to be immediately described. The marble employed here for monumental slabs appears to be peculiarly liable to the development of this crust. Another kind of white marble, sometimes employed for sculptured ornaments on tombstones, dissolves without crust. It is snowy white, and more translucent than the ordinary marble. So far as the few weathered specimens I have seen enable me to judge, it appears to be either Carrara marble, or one of the strongly saccharoid, somewhat translucent, varieties employed instead of it. This stone, however, though it forms no crust, suffers marked superficial solution. But it escapes the internal disintegration, which, so far as I have observed, is always an accompaniment of the crust. Yet the few examples of it I have met with hardly suffice for any comparison between the varieties.

(2.) *Internal Disintegration.*—Many of the marble monuments in our older churchyards are covered with a dirty crust, beneath which the stone is found on examination to be merely a loose crumbling sand. This crust seems to form chiefly where superficial solution is feeble. It may be observed to crack into a polygonal network, the individual polygons occasionally curling up so as to reveal the yellowish white crumbling material underneath. It also rises in

* For obvious reasons I withhold the names carved on the tombstones referred to in this communication.

blisters which, when they break, expose the interior to rapid disintegration.

So long as this begrimed film lasts unbroken, the smooth face of the marble slab remains with apparently little modification. The inscription may be perfectly legible. The moment the crust is broken up, however, the decay of the stone is rapid. For we then see that the cohesion of the individual crystalline granules of the marble has already been destroyed, and that the merest touch causes them to crumble into a loose sand.

It appears, therefore, that two changes take place in upright marble slabs freely exposed to rain in our burial-grounds—a superficial, more or less firm crust is formed, and the cohesion of the particles beneath is destroyed.

The crust varies in colour from a dirty grey to a deep brown black, and in thickness from that of writing paper up to sometimes at least a millimetre. One of the most characteristic examples of it was obtained from an utterly decayed tomb (erected in the year 1792) on the east side of Canongate Churchyard. No one would suppose that the pieces of flat dark stone lying there on the sandstone plinth were once portions of white marble. Yet a mere touch suffices to break the black crust, and the stone at once crumbles to powder. Nevertheless the two opposite faces of the original polished slab have been preserved, and I even found the sharply chiselled socket-hole of one of the retaining nails. The specimen was carefully removed, and soaked in a solution of gum, so as to preserve it from disintegration. On submitting the crust of this marble to microscopic investigation I found it to consist of particles of coal, grains of quartz-sand, angular pieces of broken glass, fragments of red brick or tile, and organic fibres. This miscellaneous collection of town dust was held together by some amorphous cement, which was not dissolved by hydrochloric acid. At my request my friend Mr B. N. Peach tested it with soda on charcoal, and at once obtained a strong sulphur reaction. There can be little doubt that it is mainly sulphate of lime. The crust which forms upon our marble tombstones is thus a product of the reaction of the sulphuric acid of the town-rain upon the carbonate of lime. A pellicle of amorphous gypsum is deposited upon the marble, and encloses the particles of dust which give the characteristic sooty aspect to the stone. This pellicle, of

course, when once formed, is comparatively little affected by the chemical activity of rain-water. Hence the conservation of the even surface of the marble. It is liable, however, to be cracked by an internal expansion of the stone, to which I shall immediately refer, and also to rise in small blisters, and, as I have said, its rupture leads at once to the rapid disintregation of the stone.

The cause of this disintegration is the next point for consideration. Chemical examination revealed the presence of a slight amount of sulphate in the heart of the crumbling marble; but the quantity appeared to me to be too small seriously to affect the cohesion of the stone. I submitted to microscopic examination a portion of a crumbling urn of white marble in Canongate Churchyard. The tomb bears a perfectly fresh date of 1792 cut in sandstone over the top; but the marble portions are crumbling into sand, though the structure faces the east, and is protected from vertical rain by arching mason-work. A small portion of the marble retaining its crust was boiled in Canada balsam, and was then sliced at a right angle to its original polished surface. By this means a section of the crumbled marble was obtained, which could be compared with one of the perfectly fresh stone (see fig. B). From the dark outer amorphous crust, with its carbonaceous and other miscellaneous particles, fine rifts could be seen passing down between the separated calcite granules, which in many cases were quite isolated. The black crust descends into these rifts, and likewise passes along the cleavage planes of the granules. Towards the outer surface of the stone, immediately beneath the crust, the fissures are chiefly filled with a yellowish structureless substance, which gave a feeble glimmering reaction with polarised light, and enclosed minute amorphous aggregates like portions of the crust. It probably consists chiefly of sulphate of lime. But the most remarkable feature in the slide was the way in which the calcite granules had been corroded. Seen with reflected light they resembled those surfaces of spar which have been placed in weak hydrochloric acid to lay bare enclosed crystals of zeolites. The solution had taken place partly along the outer surfaces, so as to produce the fine passages or rifts, and partly along the cleavage. Deep cavities, defined by intersecting cleavage planes, appeared to descend into the heart of some of the granules. In no case did I observe any white pellicle such as might indicate a re-deposit of

lime from the dissolved carbonate. Except for the veinings of probable sulphate just referred to, the lime, when once dissolved, had apparently been wholly removed in solution. There was further to be observed a certain dirtiness, so to speak, which at the first glance distinguished the section of crumbled marble from the fresh stone. This was due partly to corrosion, but chiefly to the introduction of particles of soot and dust, which could be traced among the interstices and cleavage lamellæ of the crystalline granules for some distance back from the crust.

It may be inferred, therefore, that the disintegration of the marble is mainly due to the action of carbonic acid in the permeating rain-water, whereby the component crystalline granules of the stone are partially dissolved and their mutual adhesion is destroyed. This process goes on in all exposures and with every variety in the thickness of the outer crust. It is distinctly traceable in tombstones that have not been erected for more than twenty years. In these which have been standing for a century it is, save in exceptionally sheltered positions, so far advanced that a very slight pressure suffices to crumble the stone into powder. But with this internal disintegration we have to take into consideration the third phase of weathering to which I have alluded. In the upright marble slabs it is the union of the two kinds of decay which leads to so rapid an effacement of the monuments.

(3.) *Curvature and Fracture.*—This most remarkable phase of rock-weathering is only to be observed in the slabs of marble which have been firmly inserted into a solid framework of sandstone, and placed in an erect or horizontal position. It consists in the bulging out of the marble accompanied with a series of fractures. This change cannot be explained as mere sagging by gravitation, for it usually appears as a swelling up of the centre of the slab, which continues until the large blister-like expansion is disrupted. Nor is it by any means exceptional; it occurs, as a rule, on all the older upright marble tablets, and is only found to be wanting in those cases where the marble has evidently not been fitted tightly into its sandstone frame. Wherever there has been little or no room for expansion, protuberance of the marble may be observed. Successive stages may be seen from the first gentle uprise to an unsightly swelling of the whole stone. This change is accompanied by fracture of the

marble. The rents in some cases proceed from the margin inwards, more particularly from the upper and under edges of the stone, pointing unmistakably to an increase in volume as the cause of fracture. In other cases the rents appear in the central part of the swelling where the tension from curvature has been greatest.

Some exceedingly interesting examples of this singular process of weathering are to be seen in Greyfriars' Churchyard. On the south wall, in the enclosure of a well-known county family, there is an oblong upright marble slab (Pl. XVI., A, measuring $30\frac{1}{4}$ inches in height, by $22\frac{3}{8}$ inches in breadth and $\frac{3}{4}$ inch in thickness, and facing west. The last inscription on it bears the date 1838, at which time, of course, it was no doubt still smooth and upright. Since then, however, it has escaped from its fastenings on either side, though still held firmly at the top and bottom. It consequently projects from the wall like a well-filled sail. The axis of curvature is, of course, parallel to the upper and lower margins, and the amount of deviation from the original vertical line is fully $2\frac{1}{2}$ inches, so that the hand and arm can be inserted between the curved marble and the perfectly vertical and undisturbed wall to which it was fixed. At the lower end of the slab a minor curvature to the extent of $\frac{1}{8}$th of an inch is observable, coincident with the longer axis of the stone. In both cases the direction of the bending has been determined by the position of the enclosing solid frame of sandstone which resisted the internal expansion of the marble. Freed from its fastenings at either side the stone had assumed a simple wave-like curve. But the tension has become so great that a series of rents has appeared along the crest of the fold. One of these has a breadth of $\frac{1}{10}$th of an inch at its opening.* Not only has the slab been ruptured, but its crust has likewise yielded to the strain, and has broken up into a network of cracks, and some of the isolated portions are beginning to curl up at the edges, exposing the crumbling decayed marble below. I should add that such has been the expansive force of the marble that the part of the sandstone block in the upper part of the frame, exposed to the direct pressure, has begun to exfoliate, though elsewhere the stone is quite sound.

* It is a further curious fact that the slab measures $\frac{1}{2}$ inch more in breadth across the centre, where it has had room to expand, than at the top, where it has been tightly jammed between the sandstone slabs.

More advanced stages of curvature and fracture may be noticed on many other tombstones in the same burying-place. One of the most conspicuous of these has a peculiar interest from the fact that it occurs on the tablet erected to the memory of one of the most illustrious dead whose dust lies within the precincts of the Greyfriars—the great Joseph Black. He died in 1799. In the centre of the sumptuous tomb raised over his grave is inserted a large upright slab of white marble, which, facing south, is protected from the weather partly by heavy overhanging masonry and partly by a high stone wall immediately to the west. On this slab a Latin inscription records with pious reverence the genius and achievements of the discoverer of carbonic acid and latent heat; and adds, that his friends wished to mark his resting-place by the marble whilst it should last. Less than eighty years, however, have sufficed to render the inscription already partly illegible. The stone, still firmly held all round its margin, has bulged out considerably in the centre, and on the blister-like expansion has been rent by numerous cracks which run, on the whole, in the direction of the length of the stone.

A further stage of decay is exhibited by a remarkable tomb on the west wall of the Greyfriars' Churchyard (Pl. XVI., B). The marble slab, bearing a now almost wholly effaced inscription, on which the date 1779 can be seen, is still held tightly within its enclosing frame of sandstone slabs, which are firmly built into the wall. But it has swollen out into a ghastly protuberance in the centre, and is, moreover, seamed with rents which strike inwards from the margins. In this and in some other examples the marble seems to have undergone most change on the top of the swelling, partly from the system of fine fissures by which it is broken up, and partly from more direct and effective access of rain. Eventually the cohesion of the stone at that part is destroyed, and the crumbling marble falls out, leaving a hole in the middle of the slab. When this takes place disintegration proceeds rapidly. Three years ago I sketched a tomb in this stage on the east wall of Canongate Churchyard (Pl. XVI., C). In a recent visit to the place I found that the whole of the marble had since fallen out.

The first cause that naturally suggests itself in explanation of the remarkable change in the structure of a substance, usually regarded as so inelastic, is the action of frost. White statuary marble is

naturally porous. It is rendered still more so by that internal solution which I have described. The marble tombstones in our graveyards are, therefore, capable of imbibing a relatively large amount of moisture. When this interstitial water is frozen, its expansive force, as it passes into the solid state, must increase the isolation of the granules and augment the dimensions of a marble block. I am inclined to believe that this must be the principal cause of the change. Whatever may be the nature of the process it is evidently one which acts from within the marble itself. Microscopic examination fails to discover any chemical transformation which would account for the expansion. Dr Angus Smith has pointed out that in towns the mortar of walls may be observed to swell up and lose cohesion from a conversion of its lime into the condition of sulphate. I have already mentioned that sulphate does exist within the substance of the marble, but that its quantity, so far as I have observed, is too small to be taken into account in this question. The expansive power is exerted in such a way as not sensibly to affect the internal structure and composition of the stone. And this I imagine is most probably the work of frost.

The results of my observations among our burial-grounds show that, save in exceptionally sheltered situations, slabs of marble, exposed to the weather in such a climate and atmosphere as that of Edinburgh, are entirely destroyed in less than a century. Where this destruction takes place by simple comparatively rapid superficial solution and removal of the stone, the rate of lowering of the surface amounts sometimes to about a third of an inch (or roughly 9 millimetres) in a century. Where it is effected by internal displacement, a curvature of $2\frac{1}{2}$ inches, with abundant rents, a partial effacement of the inscription, and a reduction of the marble to a pulverulent condition, may be produced in about forty years, and a total disruption and effacement of the stone within one hundred. It is evident that white marble is here utterly unsuited for out of door use, and that its employment for really fine works of art which are meant to stand in the open air in such a climate ought to be strenuously resisted. Of course I am now referring, not to the durability of marble generally, but to its behaviour in a large town with a moist climate and plenty of coal-smoke.

II. Sandstones and Flagstones.—These, being the common

building materials of the country, are of most frequent occurrence as monumental stones. Where properly selected they are remarkably durable. By far the best varieties are those which consist of a nearly pure fine siliceous sand, with little or no iron or lime, and without trace of bedding structure. Some of our sandstones contain 98 per cent. of silica. A good illustration of their power of resisting the weather is supplied by Alexander Henderson's tomb in Greyfriars' Churchyard. He died in 1646, and a few years afterwards the present tombstone, in the form of a solid square block of freestone, was erected at his grave. It was ordered to be defaced in 1662 by command of the Scottish Parliament, but after 1688 it was repaired. Certain bullet marks upon the stone are pointed out as those of the soldiery sent to execute the order. Be this as it may, the original chisel marks on the polished surface of the stone are still perfectly distinct, and the inscribed lettering remains quite sharp. Two hundred years have effected hardly any change upon the stone, save that on the west and north sides, which are those most exposed to wind and rain, the surface is somewhat roughened, and the internal fine parallel jointing begins to show itself.

Three obvious causes of decay in arenaceous rocks may be traced among our monuments. In the first place, the presence of a soluble or easily removable matrix in which the sand grains are embedded. The most common kinds of matrix are clay, carbonates of lime and iron, and the anhydrous and hydrous peroxides of iron. The presence of the iron reveals itself by its yellow, brown, or red colour. So rapid is disintegration from this cause that the sharply incised date of a monument erected in Greyfriars' Church to an officer who died only in 1863 is no longer legible. At least $\frac{1}{8}$th of an inch of surface has here been removed from a portion of the slab in 16 years, or at the rate of about three quarters of an inch in a century.

In the second place, where a sandstone is marked by distinct laminæ of stratification, it is nearly certain to split up along these lines under the action of the weather, if the surface of the bedding planes is directly exposed. This is well known to builders, who are quite aware of the importance of "laying a stone on its bed." Examples may be observed in our churchyards where sandstones of this character have been used for pilasters and ornamental work and

where the stone, set on its edge, has peeled off in successive layers. In flagstones, which are merely thinly bedded sandstones, this minute lamination is often fatal to durability. These stones, from the large size in which slabs of them can be obtained, and from the ease with which they can be worked, form a tempting material for monumental inscriptions. The melancholy result of trusting to their permanence is strikingly shown by a tombstone at the end of the south burying-ground in Greyfriars' Churchyard. The date inscribed on it is 1841, and the lettering that remains is as sharp as if cut only recently. The stone weathers very little by surface disintegration. It is a laminated flagstone set on edge, and large portions have scaled off, leaving a rough, raw surface where the inscription once ran. In this instance a thickness of about $\frac{1}{3}$rd of an inch has been removed in forty years.

In the third place, where a sandstone contains concretionary masses of different composition or texture from the main portion of the stone, these are apt to weather at a different rate. Sometimes they resist destruction better than the surrounding sandstone so as to be left as permanent excrescences. More commonly they present less resistance, and are therefore hollowed out into irregular and often exceedingly fantastic shapes. Examples of this kind of weathering abound in our neighbourhood. Perhaps the most curious to which a date can be assigned are to be found in the two sandstone pillars, which until recently flanked the tomb of Principal Carstares in Greyfriars' Churchyard. They were erected some time after the year 1715. Each of them is formed of a single block of stone about 8 feet long. Exposure to the air for about 150 years has allowed the original differences of texture or composition to make their influence apparent. Each column is hollowed out for almost its entire length on the exposed side into a trough 4 to 6 inches deep and 6 to 8 inches broad. As they lean against the wall, beneath the new pillars which have supplanted them, they suggest some rude form of canoe rather than portions of a sepulchral monument.

Where concretions are of a pyritous kind their decomposition gives rise to sulphuric acid, some of which combines with the iron and gives rise to dark stains upon the corroded surface of the stone. Some of the sandstones of the district, full of such impurities, ought never to be employed for architectural purposes. Every

block of stone in which they occur should be unhesitatingly condemned. Want of attention to this obvious rule has led to the unsightly disfigurement of public buildings.

III. Granites.—In Professor Pfaff's experiments, to which I have already referred, he employed plates of syenite and granite, both rough and polished. He found that they had all lost slightly in weight at the end of a year. The annual rate of loss was estimated by him as equal to 0·0076 mm. from the unpolished, and 0·0085 from the polished granite. That a polished surface of granite should weather more rapidly than a rough one is perhaps hardly what might have been expected. The same observer remarks, that though the polished surface of syenite was still bright at the end of not more that three years, it was less so than at first; and in particular, that some figures indicating the date, which he had written on it with a diamond, had become entirely effaced. Granite has been employed for too short a time as a monumental stone in our cemeteries to afford any ready means of measuring even appproximately its rate of weathering. Traces of decay in some of its felspar crystals may be detected, yet in no case that I have seen is the decay of a polished granite surface sensibly apparent after exposure for fifteen or twenty years. That the polish will disappear, and that the surface will gradually roughen as the individual component crystals are more or less easily attacked by the weather, is of course sufficiently evident. Even the most durable granite will probably be far surpassed in permanence by the best of our siliceous sandstones. But as yet the data do not exist for making any satisfactory comparison between them.

[Note added 21st May 1880. Since the preceding paper was written, I have had an opportunity of examining the condition of the monumental stones in the graveyards of a number of towns and villages in the north-east of Scotland, where the population is sparse and where comparatively little coal-smoke passes into the atmosphere. The marble tablets last longer there than in Edinburgh, but show everywhere indications of decay. They appear to be quite free from the black or grey sulphate-crust. They suffer chiefly from superficial erosion, but I observed a few cases of curvature and and fracture. As a contrast to the universal decay of the marble tombstones, reference may be made to the remarkable durability of the clay-slate which has been employed for monumental purposes

in Aberdeenshire. It is a fine-grained, rather soft rock, containing scattered cubes of pyrites, and capable of being readily dressed into thin smooth slabs. A tombstone of this material, erected in the old burying-ground at Peterhead, sometime between 1785 and 1790, retains its lettering as sharp and smooth as if only recently incised. Yet the stone is soft enough to be easily cut with the knife. The cubes of pyrites have resisted weathering so well, that a mere thin film of brown hydrous peroxide conceals the brassy undecomposed sulphide from view. The slate is slightly stained yellow round each cube or kernel of pyrites, but its general smooth surface is not affected. The lapse of nearly a century has produced scarcely any change upon this stone, while neighbouring tablets of white marble, 100 to 150 years old, present rough granular surfaces and half-effaced though still legible inscriptions.]

2. On a Realised Sulphurous Acid Steam-Pressure Thermometer, and on a Sulphurous Acid Steam-Pressure Differential Thermometer. By Sir William Thomson.

A sulphurous acid steam-pressure thermometer, on the plan described in my communication on the subject to the Royal Society of March 1, has been actually constructed, with range up to 25° C., but not yet in a permanent form. The slight trials I have been able to make with it give promise that, in respect to sensibility and convenience for practical use, it will most satisfactorily fulfil all expectations, and have given some experience in respect to the overcoming of difficulties of construction, from which the following instructions are suggested as likely to be useful to any one who may desire to make such an instrument:—

(1.) The sulphurous acid steam thermometer might more properly be called a cryometer than a thermometer, because it is not very convenient, except for measuring temperatures lower than the atmospheric temperature at the place and time of observation; for, it must be remarked, that the thermometric substance, that is to say, the infinitesimal layer of liquid and steam of sulphurous acid at the interface between the two in the bulb in the annexed drawing (fig. 1), must be at a lower temperature than any other part of the space of bulb and tube between it and the mercury

surface in the shorter vertical column. It is satisfactory, however, that the instrument is really not needed for temperatures above +10° C., because for such the water steam-pressure thermometer, represented in the first of the three diagrams of my former communication, has ample sensibility for most practical purposes. Hence, instead of the range up to 25° C. in the instrument already realised, and the great length of tube (295 centimetres for the long vertical branch) which it requires, I propose in future to let +10° be the superior limit of the temperatures to be measured by an ordinary sulphurous acid steam thermometer. For this, the long vertical branch need not be more than 175 centimetres; thus the instrument is much more easily made, and when made, is much less cumbrous.

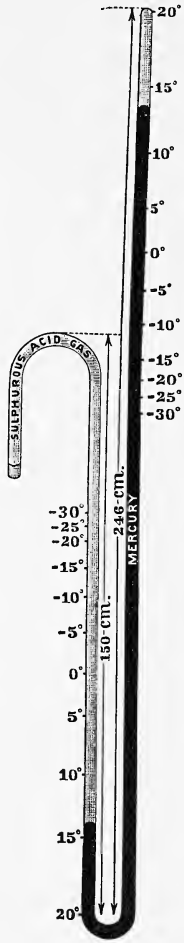


Fig. 1.

(2.) The upper end of the long branch, being open to begin with, is to be securely cemented to a small and very perfectly air-tight iron stopcock L, communicating with an iron pipe, bent at right angles, as shown in the drawing (fig. 2). This iron pipe is, in the first place, to be put into communication temporarily by an india-rubber junction with the generator, and with an air-pump, by means of a metal branch tube, with two stopcocks R and S, as shown in the drawing.

(3.) To begin, close R and open S and L; and exhaust moderately (down to half an inch of mercury will suffice). Warm the whole length of the bent tube moderately by a spirit lamp, or spirit lamps, to dry the inner surface sufficiently. Then, still maintaining the exhaustion by the air-pump, apply a freezing mixture to the bulb and shorter vertical tube, and all of the long vertical tube except a convenient length of a foot or two next its upper end, as shown in the drawing. Before joining the generator to Q, let enough of sulphurous acid gas be passed out through P to clear out fairly well the air from the generator and the purifying sulphuric acid wash-

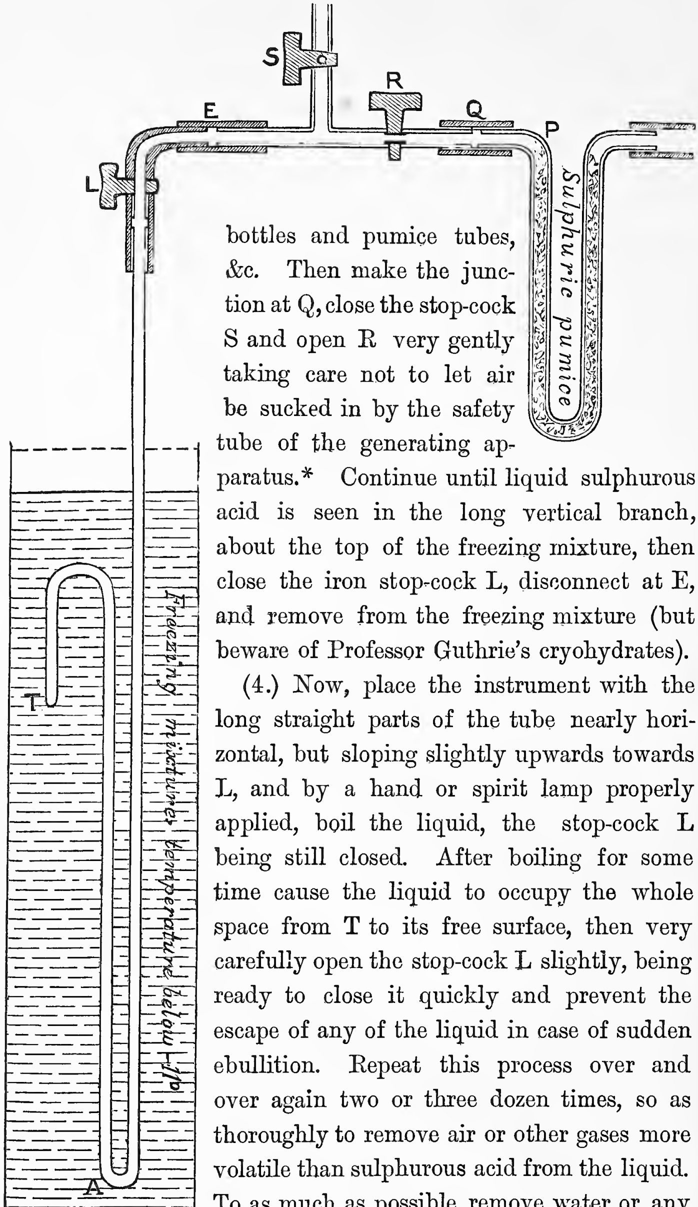


Fig. 2.

bottles and pumice tubes, &c. Then make the junction at Q, close the stop-cock S and open R very gently taking care not to let air be sucked in by the safety tube of the generating apparatus.* Continue until liquid sulphurous acid is seen in the long vertical branch, about the top of the freezing mixture, then close the iron stop-cock L, disconnect at E, and remove from the freezing mixture (but beware of Professor Guthrie's cryohydrates).

(4.) Now, place the instrument with the long straight parts of the tube nearly horizontal, but sloping slightly upwards towards L, and by a hand or spirit lamp properly applied, boil the liquid, the stop-cock L being still closed. After boiling for some time cause the liquid to occupy the whole space from T to its free surface, then very carefully open the stop-cock L slightly, being ready to close it quickly and prevent the escape of any of the liquid in case of sudden ebullition. Repeat this process over and over again two or three dozen times, so as thoroughly to remove air or other gases more volatile than sulphurous acid from the liquid. To as much as possible remove water or any

* One of the sulphuric acid wash-bottles must be provided with a safety tube with overflow bulb. An ordinary pipette with its stem fitted into the india-rubber stopper of the bottle will serve for the purpose.

fluid less volatile than sulphurous acid, proceed as follows:—Apply heat at T and in the bend next T until the liquid leaves that part of the enclosure and stands nearly at a level in the short and long vertical branch, the instrument being held with A down. Apply a freezing mixture to T, taking care not to cool it to quite as low a temperature as $-11°$ C.; so that the pressure of the sulphurous acid liquid and steam may remain something above the external atmospheric pressure. Occasionally open the stop-cock L very slightly to prevent the liquid from being drawn up the short vertical branch through preponderance of temperature in the long vertical branch. Continue this until about a centimetre of liquid has been distilled over into the bulb T. Then open the stop-cock L very carefully until all the liquid in the two vertical branches is blown out, leaving that which has been distilled over into the bulb T, and then close L again.

(6.) Then dip the end E under pure mercury, and by opening L very gently and warming the free surface of the liquid sulphurous acid, let gas escape bubbling up through the mercury. Close L again before or when the quantity of liquid in the bulb at T begins to be perceptibly diminished. Then apply a freezing mixture to T until mercury is drawn in. Incline the instrument with A up and L down, and watch until the mercury is drawn up to A, then incline with A down and let a little more mercury come in. Then close L. Lastly, keeping T still in the freezing mixture, melt the glass below L till it collapses and blows the mercury down, leaving Torricellian vacuum at the sealed end. The instrument is now complete and ready for use.

Sulphurous Acid Steam-Pressure Differential Thermometer.

This consists of a U tube, with its ends bent down, as shown in the drawing, containing mercury in the main bend and in the lower parts of the straight vertical branches, and sulphurous acid gas, steam, and liquid in the rest of the enclosure. Every other part of the enclosure must be kept somewhat warmer than the warmer of the two ends, T, T′.

The infinitesimal quantities of matter in the transitional layers, between liquid and steam, at T and T′, constitute the thermometric

substance. The gas between these and the manometric mercury, and the mercury serve merely the purpose of transmitting the steam-pressures, and measuring the difference between them.

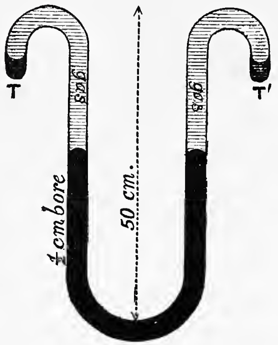


Fig. 3.

At $12\frac{1}{2}°$ C., the sensibility of the instrument, as we see by Regnault's tables of sulphurous acid steam-pressures, quoted in the article "Heat," of the eleventh volume of the *Encyclopædia Britannica*, is 7 centimetres difference of mercury levels, to 1° difference of temperature (that is temperatures 12°, 13°) at T, T′ respectively.

At $22\frac{1}{2}°$ the sensibility similarly reckoned, is 12·2 cms. to 1°.

Note on Steam-Pressure Thermometers. By Sir William Thomson.

If the bore of the vertical tube is less than three or four millimetres, there ought to be an enlargement at its upper end, or else there should not be quite enough of the liquid to fill the tall manometric tube; otherwise, if in the use of the instrument the liquid is pressed up to the top of the vertical tube, it is impossible to get it down again except by the tedious operation of distilling the whole liquid from the tube into the bulb, by applying heat by means of a spirit-lamp or a large vessel of hot water to the manometric tube, which, to facilitate this operation, may be held inclined with the closed end down. An instrument like that shown in fig. 1 of my former communication (March 1, 1880), with vertical tube of the diameter (2 or 3 millimetres) there stated is subject to this inconvenience, although, in my first attempts to realise the instrument, imperfect removal of air from the water and steam in the enclosed space prevented me from experiencing it. The difficulty, of course, might have been foreseen; but I did not think it would have been so great as I now find it to be with an instrument constructed exactly according to fig. 1 of my former communication, with air very perfectly removed from the enclosure by a proper process of boiling before sealing the instrument.

3. On a Differential Thermoscope founded on Change of Viscosity of Water with change of Temperature. By Sir Wm. Thomson.

Water flows from a little cistern or reservoir R through a wide vertical tube S, about two metres long, thence through a horizontal capillary tube C′, 50 or 100 centimetres long; thence through a wide horizontal metal tube T, 20 or 30 centimetres long; thence through a second horizontal capillary C; and lastly, out by a little constant-level overflow cup L. A vertical glass manometric tube M, a metre and a half long, standing up above the end of T next C to measure the pressure in T by a water column; and a means of giving any uniform temperature to the outsides of S and C′, and any other uniform temperature to the outsides of T and C′; complete the instrument.

Denote the heights of the levels of the water in R and M, above L, by h and $\frac{1}{2}h - x$. If C and C′ are equal and similar, or otherwise so proportioned as to be equal in their resistances to the flow of the water at equal temperatures through them, we find from the formula by which Poiseulle expressed the results of his experiments on the flow of water through capillary tubes—

$$x = \tfrac{1}{2}h \frac{\cdot 03368 \,.\, \frac{1}{2}(t - t') + \cdot 000221 \,.\, \frac{1}{2}(t^2 - t'^2)}{1 + \cdot 03368 \,.\, \frac{1}{2}(t + t') + \cdot 000221 \,.\, \frac{1}{2}(t^2 + t'^2)},$$

where t' and t denote the temperatures of the water as it flows through C′ and C. By the arrangements described it is secured that t is very nearly the same as the temperature of the outsides of B and C. Thus, if $h = 200$ cms., $t' = 0°$, and $t = 1°$, we have $x = 3 \cdot 3$. Thus the sensibility is 33 mms. per 1° C.; and 1/30 of a degree would therefore be very perceptible.

Even with its high sensibility this instrument may not be frequently found convenient for thermal researches, and its chief use may be for illustration of Poiseulle's important discovery.

4. On a Thermomagnetic Thermoscope. By Sir W. Thomson.

This thermoscope is founded on the change produced in the magnetic moment of a steel magnet by change of temperature. Several different forms suggest themselves: the one which seems best adapted to give good results is to be made as follows:—

(1.) Prepare an approximately astatic system of two thin, hardened steel wires, $r\ b$, $r'\ b'$, each 1 cm. long, one of them, $r\ b$, hung by a single silk fibre, and the other hung bifilarly from it, by fibres about 3 cms. long, so attached that the projections of the two, on a horizontal plane, shall be inclined at an angle of about ·01 of a radian (or ·57°) to one another.

(2.) Hang a very small light mirror bifilarly from the lower of the two wires.

(3.) Magnetise the two wires to very exactly equal magnetic moments in the dissimilar directions. This is easily done by a few successive trials, to make them rest as nearly as possible perpendicular to the magnetic meridian.

(4.) Take two pieces of equal and similar straight steel wire, well hardened, each 2 cms. long, and about ·04 cm. diameter; magnetise them equally and similarly; and mount them on a suitable frame to fulfil conditions (5) and (6). Call them R B and R′ B′, B and B′ denoting the ends containing true north polarity (ordinarily marked B), and R R′ true south (ordinarily marked red). The small letters r, b, r', b' mark, on the same plan, the polarities of $r\ b$ and $r'\ b'$.

(5.) The magnets R B, R′ B′, are to be relatively fixed in line on their frame, with similar poles next one another, at a distance of about 2 cms. asunder; as thus R B . . . B′ R′, with B B′ = 2 cms.

(6.) This frame is to be mounted on a geometrical slide upon the case within which the astatic pair $r\ b$, $r'\ b'$ is hung, in such a manner that the line of R B, B′ R′ bisects $r\ b$, approximately at right angles, and that R B B′ R′ may be moved by a micrometer screw through about a millimetre on each side of its central position, the line of motion being the line of R B, B′ R′, and the

"central position" being that in which B and B′ are equidistant from the centre of $r\ b$.

(7.) A lamp and scale, with proper focussing lens if the mirror is not concave, are applied to show and measure small deflections as in my mirror galvanometers and electrometer.

Use of the Thermoscope.

(8.) Place the instrument with the needles approximately perpendicular to the magnetic meridian, turning it so as to bring b and b' to the south side of the vertical plane bisecting the small angle between the projections of $r\ b$, $r'\ b$, and r and r' to the north side of it.

(9.) By aid of the micrometer screw bring the luminous image to its middle position on the scale.

(10.) Cause R B, B′ R′ to have different temperatures. The luminous image is seen to move in such a direction as is due to r approaching the cooler, and receding from the warmer of the two deflectors B R, B′ R.′

5. On a Constant Pressure Gas Thermometer. By Sir William Thomson, F.R.S.

In the article on "Heat" published in the eleventh volume of the *Encyclopædia Britannica*, referred to in my previous communications to the Royal Society on Steam Pressure Thermometers, it is shown that the Constant Pressure Air Thermometer is the proper form of expansional thermometer to give temperature on the absolute thermodynamic scale, with no other data as to physical properties of the fluid than the thermal effect which it experiences in being forced through a porous plug, as in the experiment of Joule and myself on this subject;* and the thermal capacity of the fluid under constant pressure. These data for air, hydrogen, and nitrogen have all been obtained with considerable accuracy, and therefore it becomes an important object towards promoting accurate thermometry, to make a practical working thermometer directly adapted to show temperature on the absolute thermodynamic scale through the whole range of temperature, from the lowest attainable by a r

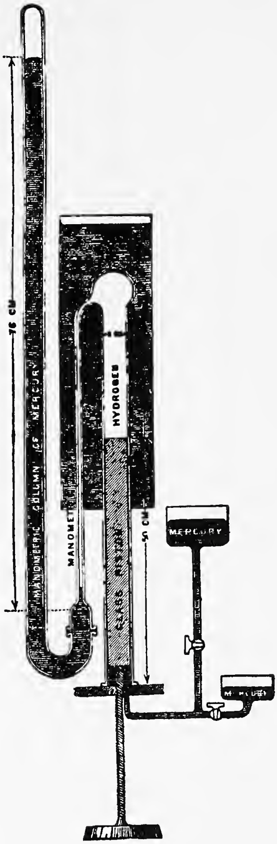
* "Thermal Effects of Fluids in Motion," Trans. Roy. Soc. Lond., June 1853, June 1854, June 1860, and June 1862.

means, to the highest for which glass remains solid. This, I believe, may be done by avoiding the objectionable expedient adopted by Pouillet and Regnault, of allowing a portion (when high temperatures are to be measured the greater portion) of the whole gas to be pressed into a cool volumetric chamber, out of the thermometric chamber proper, by the expansion of the portion which remains in; and instead fulfilling the condition, stated, but pronounced practically impossible, by Regnault ("Expériences," vol. i. pp. 168, 169), that the thermometric gas "shall like the mercury of a mercury thermometer be allowed to expand freely at constant pressure in a calibrated reservoir maintained throughout at one temperature." I have accordingly designed a constant pressure gas thermometer to fulfil this condition. It is represented in the accompanying drawing, and described in the following extract from the article referred to:—

The vessel containing the thermometric fluid, which in this case is to be either hydrogen or nitrogen,* consists in the main of a glass bulb and tube placed vertically with bulb up and mouth down; but there is to be a secondary tube of much finer bore opening into the bulb or into the main tube near its top, as may be found most convenient in any particular case. The main tube which, to distinguish it from the secondary tube, will be called the volumetric tube, is to be of large bore, not less than 2 or 3 centimetres, and is to be ground internally to a truly cylindric form. To allow this to be done it must be made of thick, well-annealed glass like that of the French glass-barrelled air-pumps. The secondary tube, which

* Common air is inadmissible, because even at ordinary temperatures its oxygen attacks mercury. The film of oxide thus formed would be very inconvenient at the surface of the mercury caulking, round the base of the piston, and on the inner surface of the glass tube to which it would adhere. Besides, sooner or later the whole quantity of oxygen in the air must be diminished to a sensible degree by the loss of the part of it which combines with the mercury. So far as we know, Regnault did not complain of this evil in his use of common air in his normal air thermometer nor in his experiments on the expansion of air ("Expériences," vol. i.), though probably it has vitiated his results to some sensible degree. But he found it to produce such great irregularities when, instead of common air, he experimented on pure oxygen, that from the results he could draw no conclusion as to the expansion of this gas ("Expériences," vol. i. p. 77). Another reason for the avoidance of air or other gas containing free oxygen is to save the oil or other liquid which is interposed between it and the mercury of the manometer from being thickened or otherwise altered by oxidation.

will be called the manometric capillary, is to be of round bore, not very fine, say from half a millimetre to a millimetre diameter. Its lower end is to be connected with a mercury manometer to show if the pressure of the thermometric air is either greater or less than the definite pressure to which it is to be brought every time a thermometric measurement is made by the instrument. The change of volume required to do this for every change of temperature is made and measured by means of a micrometer screw* lifting or lowering a long solid glass piston, fitting easily in the glass tube, and caulked air-tight by mercury between its lower end and an iron sole-plate by which the mouth of the volumetric tube is closed. To perform this mercury caulking, when the piston is raised and lowered, mercury is allowed to flow in and out through a hole in the iron sole-plate by an iron pipe, connected with two mercury cisterns at two different levels by branches each provided with a stopcock. When the piston is being raised the stopcock of the branch leading to the lower cistern is closed, and the other is opened enough to allow the mercury to flow up after the piston and



* This screw is to be so well fitted in the iron sole-plate as to be sufficiently mercury-tight without the aid of any soft material, under such moderate pressure as the greatest it will experience when the pressure chosen for the thermometric gas is not more than a few centimetres above the external atmospheric pressure. When the same plan of apparatus is used for investigation of the expansion of gasses under high pressures, a greased leather washer may be used on the upper side of the screw-hole in the sole-plate, to prevent mercury from escaping round the screw. It is to be remarked that in no case will a little oozing out of the mercury round the screw while it is being turned introduce any error at all into the thermometric result; because the correctness of the measurement of the volume of the gas depends simply on the mercury being brought up into contact with the bottom of the piston, and not more than just perceptibly up between the piston and volumetric tube surrounding it.

press gently on its lower side, without entering more than infinitesimally into the space between it and the surrounding glass tube (the condition of the upper bounding surface of the mercury in this respect being easily seen by the observer looking at it through the glass tube). When the piston is being lowered, the stopcock in the branch leading from the upper cistern is closed, and the one in the branch leading to the lower cistern is opened enough to let the mercury go down before the piston, instead of being forced to any sensible distance into the space between it and the surrounding tube, but not enough to allow it to part company with the lower surface of the piston. The manometer is simply a mercury barometer of the form commonly called a siphon barometer, with its lower end not open to the air but connected to the lower end of the manometric capillary. This connection is made below the level of the mercury in the following manner. The lower end of the capillary widens into a small glass bell or stout tube of glass of about 2 centimetres bore and 2 centimetres depth, with its lip ground flat like the receiver of an air-pump. The lip or upper edge of the open cistern of the barometer (that is to say, the cistern which would be open to the atmosphere were it used as an ordinary barometer) is also ground flat, and the two lips are pressed together with a greased leather washer between them to obviate risk of breaking the glass, and to facilitate the making of the joint mercury tight. To keep this joint perennially good, and to make quite sure that no air shall ever leak in, in case of the interior pressure being at any time less than the external barometric pressure or being arranged to be so always, it is preserved and caulked by an external mercury jacket not shown in the drawing. The mercury in the thus constituted lower reservoir of the manometer is above the level of the leather joint, and the space in the upper part of the reservoir over the surface of the mercury, up to a little distance into the capillary above, is occupied by a fixed oil or some other practically vapourless liquid. This oil or other liquid is introduced for the purpose of guarding against error in the reckoning of the whole bulk of the thermometric gas, on account of slight irregular changes in the capillary depression of the border of the mercury surface in the reservoir.

In the most accurate use of the instrument, the glass and mercury and oil of the manometer are all kept at one definite temperature, according to some convenient and perfectly trustworthy intrinsic thermoscope, by means of thermal appliances not represented in the drawing but easily imagined. This condition being fulfilled, the one desired pressure of the thermometric gas is attained with exceedingly minute accuracy by working the micrometer screw up or down until the oil is brought precisely to a mark upon the manometric capillary.

In fact, if the glass and mercury and oil are all kept rigorously at one constant temperature, the only access for error is through irregular variations in the capillary depressions in the borders of the mercury surfaces. With so large a diameter as the 2 centimetres chosen in the figured dimensions of the drawing, the error from this cause can hardly amount to $\frac{1}{100}$ per cent. of the whole pressure, supposing this to be one atmo or thereabouts.

For ordinary uses of this constant-pressure gas thermometer, where the most minute accuracy is not needed, the rule will still be to bring the oil to a fixed mark on the manometric capillary; and no precaution in respect to temperature will be necessary except to secure that it is approximately uniform throughout the mercury and containing glass, from lower to higher level of the mercury. The quantity of oil is so small that, whatever its temperature may be, the bringing of its free surface to a fixed mark on the capillary secures that the mercury surface below the oil in the lower reservoir is very nearly at one constant point relatively to the glass, much more nearly so than it could be made by direct observation of the mercury surface, at all events without optical magnifying power. Now if the mercury surface be at a constant point of the glass, it is easily proved that the difference of pressures between the two mercury surfaces will be constant, notwithstanding considerable variations of the common temperature of the mercury and glass, provided a certain easy condition is fulfilled, through which the effect of the expansion of the glass is compensated by the expansion of the mercury. This condition is, that the whole volume of the mercury shall bear to the volume in the cylindric vertical tube from the upper surface to the level of the lower surface the ratio of $(\lambda - \frac{1}{3}\sigma)$ to $(\lambda - \sigma)$, where λ denotes the cubic expansion of the mercury and σ the cubic expansion

of the solid for the same elevation of temperature, it being supposed for simplicity of statement that the tube is truly cylindric from the upper surface to the level of the lower surface, and that the sectional area of the tube is the same at the two mercury surfaces. The cubic expansion of mercury is approximately seven times the cubic expansion of glass. Hence

$$(\lambda - \tfrac{1}{3}\sigma)/(\lambda - \sigma) = (7 - \tfrac{1}{3})/6 = 1{\cdot}111\,.$$

Hence the whole volume of the mercury is to be about 1·111 times the volume from its upper surface to the level of the lower surface; that is to say, the volume from the lower surface in the bend to the same level in the vertical branch is to be $\frac{1}{9}$ of the volume in the vertical tube above this surface. A special experiment on each tube is easily made to find the quantity of mercury that must be put in to cause the pressure to be absolutely constant when the surface in the lower reservoir is kept at a fixed point relatively to the glass, and when the temperature is varied through such moderate differences of temperature as are to be found in the use of the instrument at different times and seasons.

A sheet-iron can containing water or oil or fusible metal, with external thermal appliances of gas or charcoal furnace, or low-pressure or high-pressure steam heater, and with proper internal stirrer or stirrers, is fitted round the bulb and manometric tube to produce uniformly throughout the mass of the thermometric gas the temperature to be measured. This part of the apparatus, which will be called for brevity the heater, must not extend so far down the manometric tube that when raised to its highest temperature it can warm the caulking mercury to as high a temperature as 40° C., because at somewhat higher temperatures than this the pressure of vapour of mercury begins to be perceptible, and would vitiate the thermometric use of the pure hydrogen or nitrogen of our thermometer. To secure sufficient coolness of the mercury it will probably be advisable to have an open glass jacket of cold water (not shown in the drawing) round the volumetric tube, 2 or 3 centimetres below the bottom of the heater, and reaching to about half a centimetre above the highest position of the bottom of the piston.

It seems probable that the constant-pressure hydrogen or nitrogen

gas thermometer which we have now described may give even more accurate thermometry than Regnault's constant-volume air thermometers, and it seems certain that it will be much more easily used in practice.

We have only to remark here further that, if Boyle's law were rigorously fulfilled, thermometry by the two methods would be identical, provided the scale in each case is graduated or calculated so as to make the numerical reckoning of the temperature agree at two points,—for example, 0° C. and 100° C. The very close agreement which Regnault found among his different gas thermometers and his air thermometers with air of different densities, and the close approach to rigorous fulfilment of Boyle's law which he and other experimenters have ascertained to be presented by air and other gases used in his thermometers, through the ranges of density, pressure, and temperature at which they were used in these thermometers, renders it certain that in reality the difference between Regnault's normal air thermometry and thermometry by our hydrogen gas constant-pressure thermometer must be exceedingly small. It is therefore satisfactory to know that for all practical purposes absolute temperature is to be obtained with very great accuracy from Regnault's thermometric system by simply adding 273 to his numbers for temperature on the centigrade scale. It is probable that at the temperatures of 270° or 300° C. (or 532 or 573 absolute) the greatest deviation of temperature thus reckoned, from correct absolute temperature, is not more than half a degree.

Monday, 3d May 1880.

PROFESSOR DOUGLAS MACLAGAN, Vice-President, in the Chair.

KEITH PRIZE.

The Chairman announced that the Council had awarded the Keith Prize, for the biennial period 1877–79, to Professor H. C. Fleeming Jenkin, for his Paper "On the Application of Graphic Methods to the Determination of the Efficiency of Machinery," published in the Society's Transactions; Part II. having appeared in the volume for 1877–78.

The following Communications were read :—

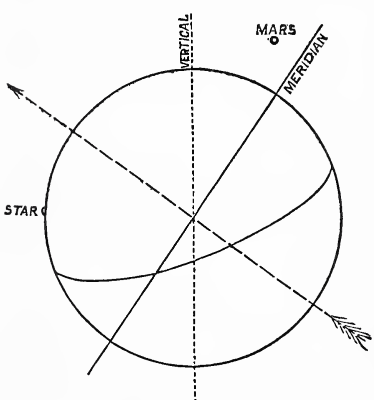
1. On the Occultation of the Star 103 Tauri. (B. A. C. 1572.) By Edward Sang.

An occultation of a star, though not appealing to ordinary observation with the same force, is intrinsically an event as striking as an eclipse of the sun. It establishes the fact of the moon's proximity. Were it not that the moon's brightness overpowers the light of the small stars, occultations would be commonplace phenomena. As things are, we can watch, with the eye unaided, the eclipses of the planets and larger stars, not down, perhaps, to below the third magnitude; and the rarity of such conspicuous objects makes the occultations correspondingly rare.

By help of a telescope of two or three feet in focal length, we are able to examine stars even so small as of the sixth magnitude, and thus greatly to increase the number of observations, so much so that as many as 150 occultations may be visible from one place in the course of the year.

The particular case to which I would draw attention is thus one of many: it derives its interest from the proximity of the planet Mars, whose occultations will have been carefully observed from many places.

The three objects—the moon, Mars, and the star—were all within the field of the telescope; their relative positions at the instant of the star's disappearance being as shown in the accompanying figure. The observation was made with a telescope having an aperture of 1·9 in., a focal length of 23·5 in., and a magnifying power of 26. The moon's dark edge was distinctly visible, the atmospheric tremor was slight, so that, notwithstanding the moon's proximity to the horizon, the disappearance was watched under very favourable circumstances. The time was noted by an excellent chronometer, which was compared, twelve hours thereafter,



with the mean time clock of the Royal Observatory, and again rated after nine days. The comparison of the time may be held as true to within a quarter of a second. The observation may be recorded thus:—

North Latitude,	. . .	55° 55′ 42″
West Longitude,	. . .	0h. 12m. 42s.

Greenwich mean solar time of disappearance—

1880, March, 17d. 12h. 23m. 55·8s.,

as by the mean time clock of the Royal Observatory of Edinburgh.

In order to compare this with the predicted motions, the moon's right ascension and declination were interpolated strictly from the hourly table in the "Nautical Almanac," and the star's place was taken from the "Elements of Occultations," while the parallax was computed on the supposition that the earth's equatorial is to the polar axis as 300 to 299. In this way the expected time was computed to be 17d. 12h. 23m. 46·3s., or 9·5s. earlier than the observed time.

There is no astronomical phenomenon more definite as to time than the occultation of a star, nor any perhaps more easily observed when the disappearance is against the dark edge of the moon. Provided that the telescope be sufficiently powerful to show the star, it is of little or no moment whether the definition be good, or even whether the instrument have been well adjusted to focus. In all cases the disappearance is instantaneous.

But, although the observation be thus satisfactory, there are various difficulties in the way of the calculations. In the first place, there is the error to which our lunar tables are liable; these tables, all founded on previous observation, are brought forward by an estimate of the laws and rate of change, and thus are unavoidably subject to a gradually increasing uncertainty. Wonderfully exact as these tables are, it would always be necessary, before drawing any exceedingly minute conclusion, to study the tabular error as obtainable from nearly contemporaneous observations on the moon. Next we have the possible error in the tabulated place of the star; an error, in the case of small stars, which is not to be

despised. The number of such stars is great, the number of observers small.

Next in order comes the size and shape of the earth. A relatively small difference in our position on the earth's surface makes a notable difference on the apparent position of the moon, and, in consequence, on the time of the occultation; even the height of the observer above the level of the sea has its influence. This may be well studied in the present instance; viewed from our latitude, the moon was seen to pass a little to the south of the planet Mars, whereas in the southern counties of England an occultation was seen. The oblateness of the earth has also to be taken into account. In the present instance calculations made as if the earth were spherical, would give the disappearance some eighteen seconds earlier than the above. Hence observations made on these phenomena from places in different latitudes afford a means for determining the earth's oblateness.

But, lastly, these observations are all deranged by the extreme jaggedness of the moon's edge. This jaggedness is well seen during an eclipse of the sun; it is also conspicuous against the disc of a planet. I recollect of witnessing an occultation of Saturn, some half a century ago, during which the corner of a lunar mountain was projected against the planet in such a way as to cut out a sector of about one-third of the surface. Irregularities of such magnitude cause serious variations in the times of disappearance and reappearance; and, for the purpose of estimating their possible extent, it might be useful to make concerted observations at places a few miles apart, so that the appulse may happen, here on the top of a lunar mountain, there in the hollow.

2. On Currents produced by Friction between Conducting Substances, and on a new form of Telephone Receiver. By James Blyth, M.A.

In former papers laid before this Society, I showed that when any two metals are rubbed against each other, a current of electricity is produced; and that this current agrees in direction with the thermo-electric current for the same two metals, and is greater, approximately at least, in proportion as the metals rubbed are far

apart on the thermo-electric scale,—the greatest current, as far as I have yet observed, being got from antimony and bismuth.

It is very difficult to decide as to the cause or causes of such currents. They may be (1) purely thermo-electric; (2) the currents, which are the supposed cause of friction; (3) currents produced by contact force between adhering films of air, moisture, or other substances with which the surfaces rubbed are tarnished; or (4) they may arise from all these causes combined. The following experiments were made in hopes of getting some information on these points.

My first experiment was to obtain the exact difference, as far as the production of a momentary current is concerned, between rubbing two pieces of metal together, and knocking the one against the other. For this purpose I repeated, with greater care, an experiment which I formerly described. It consisted in attaching a wire firmly to an ordinary hammer, and leading it to one of the erminals of a telephone circuit, while the wire from the other terminal was rigidly attached to a stiff bar of copper held vertically in a small table vice. When the face of the hammer was rubbed against the end of the copper bar, a very distinct grating noise was always heard in the receiving telephone; but the sound was almost inaudible when the bar was knocked by the hammer, if proper care were taken not to combine rubbing with knocking. This is, however, so difficult practically, that it is just possible that the sounds which I heard are due to faint rubs accompanying the knocking.

Should this not be the case, however, this difference of effect seems to show that the currents are not wholly, although they may be mainly, thermo-electric, as it is hard to believe that the heat produced at the junction of the surfaces by a smart blow can be less than that produced by a faint rub. Granting that the knocking is actually heard, it seems not unlikely that this effect may be due to the currents associated with rapid changes of form in matter. As has been remarked by Professor Tait (Proc. Roy. Soc. Edin. vol. ix. p. 552), these currents are such as would be capable of detection by the telephone.

In order to detect what effect, if any, the presence of the air had upon these friction currents, I employed the apparatus commonly called the *electric egg*. Having unscrewed the interior balls, I

fastened in their places two metallic strips, one of copper and the other of iron, so arranged that they could be made to rub against each other by moving the upper rod up and down in its air-tight socket. Before being fixed on, the metal surfaces were both well cleaned by scraping. When this apparatus was included in the circuit either of a galvanometer or telephone, no difference could be detected either in the deflection or the sound produced, by exhausting the air, as far as could be done, with an ordinary good air-pump. It is possible, however, that there may be films of air adhering to the metals which cannot be removed by pumping. Indeed, in the whole of this inquiry, the great difficulty is to be sure of what are the surfaces that are in contact.

Having ascertained that the current produced by the friction of antimony and bismuth is of some strength, and fairly constant when the friction is constant, I proceeded to make a small dynamo machine for producing currents on this principle. It consists of a cylinder of antimony 3 inches long and $2\frac{1}{2}$ inches in diameter, mounted on an axis which runs in centres. By a fly-wheel and band this cylinder is driven rapidly round against a plate of bismuth pressed tight against it by a stiff spring. Wires are led from the plate of bismuth and from one of the pivots on which the cylinder revolves to two binding screws, which form the electrodes of the machine. When this machine is included in the circuit with a microphone transmitter and a telephone, the current from it can be used for the transmission of musical sounds and even loud speaking. There is, however, heard along with the transmitted sound the noise arising from the friction of the antimony and bismuth. I have succeeded in transmitting, in this way, very distinctly, tunes played on a violin to which a microphone was attached. It is very curious, in this experiment, to hear so distinctly the music, notwithstanding the friction noise which accompanies it. It is to be noticed that the sound heard in the telephone of the rubbing of two pieces of metal together in a distant room is an effect precisely identical to this. In this case the rubbing produces the current, and the more or less loose contact of the metals acts as the microphone whereby the sound is transmitted through means of that current to the telephone.

I have also tried, with varying success, several other forms of this *friction current-producer*. In one of the most effective of these the

rubbing substances are arranged like a pair of mill-stones, the lower stone being a disc of iron laid horizontally, and the upper a disc of copper mounted on a vertical axis on which it can revolve. The surfaces are kept pressing against each other by a strong spring. When the upper disc is made to revolve rapidly, a very decided current is produced; and this I found to be markedly increased, as indicated by the telephone, by feeding in between the discs powdered antimony and bismuth combined. Of course we have here a series of rapid reversals of the current, as the direction of the current will depend upon whether particles of antimony or particles of bismuth are in contact with the lower plate. This clearly indicates a thermo-electric effect; and I have no doubt that the effect will be increased by applying a means whereby the upper surface of the copper plate and the lower surface of the iron one can be kept cold by a freezing mixture. As yet, however, I have not had time to try that. In another form I took two cylinders, the one of antimony and the other of bismuth, and placed them together end-wise, the pressure between them being regulated by a screw. The antimony cylinder was kept stationary, and the bismuth made to revolve very rapidly against it, so much so that both cylinders rapidly became hot. This also gave a pretty strong current.

Seeing that the friction between metals does certainly produce an electric current, it seemed natural to inquire whether an electric current sent from a battery across the surface between two metals would not modify the friction of the one against the other. I have tried to test this in a variety of ways, and the results leave me in doubt whether to attribute the indications which I have received to actual changes in the friction or to incipient fusion of portions of the surfaces together by the heat produced by the current, or to an effect similar to the Trevelyan rocker. In one experiment I made an inclined plane which carried a pair of parallel rails of copper about three quarters of an inch apart. The rails were hinged at the lower end, so that the plane could be set at any angle with the horizon. It was so arranged that the current from the battery could be sent up the one rail, through any conductor laid across the two, and down the other rail. The surfaces of the rails were made quite smooth. When a heavy piece of metal was laid across the rails, the angle of repose was the same both when there was and was not a

current passing. It was different, however, when a light body, such as a sewing needle, was put on. Then, when the current from three Bunsen cells was passing, the plane could be elevated considerably past the angle of repose for no current, before the needle rolled down. On examination I found that the needle was actually sticking to the copper; but that, in almost all cases, this sticking gave way without the angle being altered after the current had been taken off for some time, and the needle and copper allowed to come back to their normal temperature. In another experiment I employed a Bell telephone to enable me to detect any variation of friction when a current was passing between the rubbing surfaces. To the centre of the telephone disc was attached a long narrow strip of light wood; the object of making the strip so long being to remove the telephone as far as possible from the inductive action of the battery current which was to be used. To the other end of the strip was attached a flat piece of bismuth. This rested on the convex surface of a cylinder of antimony, which could be rapidly rotated. The battery current was sent through the antimony and bismuth by entering the antimony by the axis on which it revolved, and leaving the bismuth by a spring pressing tightly against it. In the battery circuit was included the violin with its microphone already mentioned, and the telephone with the rod attached was placed as the transmitting telephone in a telephone circuit. When the antimony cylinder was rapidly rotated, a listener in the receiving telephone watched attentively till his ear became accustomed to the sound produced by the rubbing, and transmitted along the wooden rod to the telephone disc. The battery circuit was then joined, and the violin played, the antimony cylinder meanwhile rotating at the same rate as before. No alteration in the sound was audible, which indicated no alteration in the friction. I then substituted a sharp point for the flat piece of bismuth, and immediately the violin sounds were faintly but clearly heard. This led me to think that some sticking was produced by the fusing of the sharp point, and more especially as the sound became a little clearer as the rotation became very slow.

Acting on this hint, it immediately occurred to me that a receiving telephone could be constructed depending upon this effect. I therefore took my bismuth cylinder and mounted it on a frame so that it could be made to rotate very truly on pivots. By wheels

and bands it was also made to rotate slowly. A phonograph mouthpiece, with a very thin disc of wood or mica, was next placed, so that a fine wire with a sharp point bent at a right angle, and with its other end attached to the centre of the disc just pressed with its sharp point on the convex surface of the bismuth cylinder. A current of four Bunsen cells was now passed through the wire and cylinder, and also through the violin microphone. When the violin was played the tune was heard faintly proceeding from the mouthpiece even when the bismuth cylinder was stationary. This arose simply from the loose contact of the wire and bismuth. The sound was, however, very greatly increased when the cylinder was rotated slowly,—so loud indeed, that it could be distinctly heard all over an ordinary room. I have been able to transmit singing very clearly, but not speaking clearly enough to be understood. This instrument is analogous to the loud-speaking telephone of Mr Edison; but the explanation of their action must be very different if electrolysis, as is usually supposed, be the cause of the variation in the slipping of the platinum point on the chalk cylinder, which is characteristic of Edison's instrument. Quite recently the electrolytic action has been questioned, and a different explanation given by Professor Barret of Dublin. It is evident that electrolysis can in no sense come into play when the cylinder and rubbing point are both metallic. In that case two probable explanations of the action readily suggest themselves. The one is that there is more or less of an actual sticking of the metals together, arising from their fusion by the heat of the current. If this be so, then, the loose contact is alternately made a very good one, and then one actually broken. The other is the action of the Trevelyan rocker. Here, however, we have clearly only an analogous, and not by any means an identical effect. In the Trevelyan rocker the heat passes from a large mass of hot metal through two points of contact to a cold block, whereas, in the other case the heat is only produced at the surfaces of separation, the temperature of the rest of the metals being almost unaffected. Still it appears to me that the variations of the heat at this point has a great deal to do with the actions of all microphones, and in general with all sounds transmitted from one loose contact to another. This is shown by substituting cylinders of different metals for the bismuth cylinder above mentioned, all other things remain-

ing the same. I have tried in this way, besides bismuth, cylinders of lead, tin, iron, antimony, and carbon, and find that bismuth gives by far the best result. In the other cases the sound from the simple loose contact is heard clearly enough; but there is hardly any increase of it produced by rotating the cylinder. Now this seems to be due in great part, if not entirely, to the difference between the metals as regards their specific heat and thermal conductivity. Obviously, with the same current, the greatest heat will be produced at the junction of the rubbing point and cylinder, when the specific heat and thermal conductivity are both as low as possible. Hence very probably the reason why bismuth answers so well, seeing that of all common metals it stands lowest on the list both in specific heat and thermal conductivity. In fact, if we take the product of the reciprocals of the specific heats and thermal conductivities of the above-mentioned metals, we find the product for bismuth greatly in excess of that for any of the others.

3. Note on the Present Outbreak of Solar Spots. By Professor Piazzi Smyth.

4th April 1880.

The physical activity going on in the Sun is still increasing, and worthy of all admiration. There was a very large spot had come round on the North-following limb on March 29; and was after that the subject of observation from day to day as it approached the central Solar meridian. But when it arrived there, on April 3, behold two less large but still most notable spots had burst out clear and full within the previous twenty-four hours between the great spot and the preceding limb. And on this day, April 4, there are two more notable ones very close to the greatest spot, making in all five remarkable spots not only all visible at once, but working and seething positively before our eyes.

April 5, Noon. Three of yesterday's five spots are gone. Faculæ are in their place; and that is "the end of spot-life," says Prof. Alex. S. Herschel.

4. Positive and Negative Electric Discharge between a Point and a Plate and between a Ball and a Plate. By Alexander Macfarlane, M.A., D.Sc., F.R.S.E.

I have made the following observations in the Natural Philosophy classroom of the United College, St Andrews, with the view of ascertaining whether the electromotive force required to cause a spark to pass between a small globe and a plate, or between a point and a plate, differs for the two kinds of electricity. Sir William Thomson suggested that I should apply to this question the method of measuring the electromotive force required to produce sparks, which I have described in papers already contributed to the Society (Trans. Roy. Soc. Edin. vol. xxviii. p. 633). It is a problem to which Faraday attached great importance. He says at sect. 1523, vol. 1, of his *Experimental Researches in Electricity*: "The results connected with the different conditions of positive and negative discharge will have a far greater influence on the philosophy of electrical science than we at present imagine, especially if, as I believe, they depend on the peculiarity and degree of polarised condition which the molecules of the dielectrics concerned acquire." He records a great number of experiments on this subject in sections 1465–1525. He took sparks between a ball 0·25 inch in diameter and a ball 2 inches in diameter. When the large one was connected with a discharging train, the small one charged positively gave a much longer spark than when charged negatively; also the small ball charged negatively gave a brush more readily than when charged positively in relation to the effect produced by increasing the distance between the two balls (sect. 1489). When the interval was below 0·4 of an inch, so that the small ball gave sparks whether positive or negative, he could not, he says, observe any constant difference either in their ready occurrence or the number which passed in a given time. But when the interval was such that the small ball when negative gave a brush, then the discharges from it as separate negative brushes were far more numerous than the corresponding discharges from it when rendered positive, whether those positive discharges were as sparks or brushes (sect. 1490).* As he puts

* Drs De La Rue and Müller have found in the case of the discharge of their great chloride of silver battery that the discharge between a point

it further on, he found difficulty in determining "the relative degrees of charge which the small ball acquires before discharge occurs, that is, whether it acquires a higher condition in the negative, or in the positive state, immediately preceding that discharge." The method of our experiments, it appears to me, has solved this difficulty.

I employed a Thomson's divided ring reflecting electrometer, which has either half-ring insulated. In this instrument Professor Swan has substituted a glass dish with a tubulure for holding the sulphuric acid in place of the original leaden pan, in which the charge of acid was carried on pieces of pumice stone. The tubulure allows the acid to be filled in or taken out with convenience. A piece of platinum foil suspended in the acid, from the aluminium needle, completely checks the oscillations, and renders the instrument "dead beat." The terminals for connecting the divided rings, which originally projected downwards, now project upwards, and, to secure perfect contact, are in the form of a helical brass spring passing through a varnished glass tube. The instrument was set so that the needle when charged was symmetrically situated with respect to the two half-rings, and the scale was set so that the wire-image was in the middle. The Holtz machine by which the electricity was produced, was at a distance of 20 feet from the electrometer.

In the first series of observations the point used was the conical point of a rod of a Henley's discharger. It was connected by means of an insulated wire with one conductor of the Holtz machine. The plate used is of 7 inches diameter, and was attached to the other stem of the discharger in such a manner that the rod was always normal to it at its centre. Both the plate and the other conductor of the Holtz machine were in conducting connection with the earth. Either kind of electricity was obtained by charging the one or the other paper conductor of the machine by means of a small Leyden jar. As either half ring of the electrometer could be insulated and the other connected with the earth, readings were taken in both directions. This is the meaning of the entries in the record *connections direct* and

and a disc is much more continuous with the point negative than with the point positive (Phil. Trans. vol. clxix. p. 90).

connections reversed. The *deflection* in the table is the position of the wire-image immediately before the passing of the spark; and the *zero* is its position after the spark had passed, and the Leyden jars of the Holtz machine had been discharged. These jars were found to contain a residual charge, but its existence had only a very slight effect upon the position of the wire-image.

In the second series of observations, the arrangement differed only in this—that a spherical ball of ·5 inch diameter was attached to the end of the rod.

From the first series of observations, by taking the mean of the two opposite readings, we obtain the following results:—

POINT AND PLATE.

Distance between Point and Plate.	Electromotive force for Positive Discharge (1).	Electromotive force for Negative discharge (2).	Ratio of (1) to (2).
½ inch	76·6	67·1	1·14
1 inch	86·3	76·2	1·13
2 inches	102·3	95·2	1·07

Thus the electromotive force for the positive discharge was always greater than for the negative; but the ratio approaches the more nearly to unity the greater the distance of the point from the large plate. Thus the difference in the electromotive forces appears to be due to the presence of the large uninsulated plate. The behaviour of the index showed that the discharge was not single, but consisted of a rapid succession of discharges; for it first attained a temporary maximum deflection and then a steady deflection slightly less than the maximum. The latter on account of its being capable of being observed with greater precision was the one recorded. The discharge was not silent, but accompanied with a slight hissing sound.

From the second table, by taking the mean of the two opposite readings, we obtain the following results:—

BALL AND PLATE.

Length of Spark.	Electromotive force for Positive Spark (1).	Electromotive force for Negative Spark (2).	Ratio of (1) to (2).
$\frac{1}{4}$ inch	118·8	129·7	·92
$\frac{1}{2}$ inch	179·6	201·7	·89
$\frac{3}{4}$ inch	219·2	227·3	·96
1 inch	234·6	234·3	1·00

Thus under the above conditions and for a range of spark up to ·5 inch, at least, the electromotive force required to produce the discharge is less when the ball is charged positively than when charged negatively. Within that range the discharge took place in the form of a single loud white spark, the index gave only one reading, and fell back after the passage of the spark almost to its ultimate position. But when the distance between the extremity of the ball and the plate was increased to ·75 inch, the charge being negative, hissing sparks, giving only very small discharges as indicated by the behaviour of the index, preceded the loud spark which gave complete discharge. When the charge was changed to positive, the distance remaining the same, no hissing discharges were observed preceding the loud discharge. This is an instance of the phenomenon to which Faraday refers, viz., that when the charged ball (under the above conditions) is positive, a longer spark can be obtained than when the charged ball is negative. When the distance was increased to 1 inch, hissing discharges preceded the loud discharge in both cases, but they were much more numerous in the case of the negative than of the positive charge.

The results of the observations appear to explain this phenomenon. They show that a charge of positive electricity requires a less electromotive force than a charge of negative, in order to force its way in the form of a spark (which is a complete discharge). A charge of positive electricity will therefore be able to discharge all together at a greater distance, provided we assume that the brushy spark begins at the same electromotive force for each.

The truth or falsity of this assumption appears to be capable of being established with ease by means of the method of these experiments.

From the end of the second table it appears that the ratio tends to change from being less than unity to being greater than unity, when the hissing discharges begin to appear. Suppose the negative spark preceded by hissing discharges, but the positive not. Then the occurrence of these hissing discharges is apt to diminish the deflection at the time when the negative spark passes, while their absence in the case of the positive spark allows the full deflection to be observed. Thus the ratio of the readings may change to be greater than unity. In the case of the positive spark, the electromotive forces for the four distances lie well on the curve which from previous experiments we found to be true for the discharge between a ball and a plate, but in the case of the negative spark, only those for the first two distances.

The discharge from the point is a more complex phenomenon than the discharge from the ball; its explanation probably requires many further experiments.

These results accord with those published by Drs De La Rue and Müller, in Part I. of their "Research." They state (Phil. Trans. vol. clxix. p. 76) that with high tensions—5000 to 8000 chloride of silver cells—the spark between a point and a disc is longer when the point is positive, but with low tensions up to 3000 cells, it is generally longer when the point is negative. For the discharge they observed is single, like that which we obtained between the ball and plate, at the smaller distances, not intermittent like that which we obtained between the point and plate.

I may mention that the observations of April 10, 1880, are supported by a previous series of observations, taken by means of a more roughly divided scale.

The inductric and inducteous balls (to employ terms invented by Faraday), by which the measurement was effected, were at a distance from one another of 15 inches; and the length of the wire connecting the inducteous ball with the electrometer was about 10 feet.

In the record of observations the entries are given in the order in which they were observed.

RECORD OF OBSERVATIONS.

10*th April* 1880.

Table I.—Point and Plate.

Length of Discharge.	Charge on Point.	Connections of Electrometer.	Deflection.	Zero.	Difference.	Mean.	Ratio of Positive to Negative.
½ inch	positive	direct	279	355	76		
,,	,,	,,	282	360	78	77·7	
,,	,,	,,	290	369	79		
,,	,,	reversed	425	350	75		
,,	,,	,,	423	346	77	75·5	1·12
,,	,,	,,	421	347	74		
,,	negative	,,	265	335	70		
,,	,,	,,	264	332	68	69·3	1·16
,,	,,	,,	261	331	70		
,,	,,	direct	423	358	65		
,,	,,	,,	427	362	65	65·0	
,,	,,	,,	430	365	65		
1 inch	negative	direct	453	378	75		
,,	,,	,,	454	377	77	75·5	
,,	,,	,,	457	383	74		
,,	,,	reversed	265	342	77		
,,	,,	,,	265	342	77	77·0	1·16
,,	,,	,,	265	342	77		
,,	positive	,,	443	353	90		
,,	,,	,,	440	352	88	88·0	1·10
,,	,,	,,	438	352	86		
,,	,,	direct	263	344	81		
,,	,,	,,	260	349	89	84·7	
,,	,,	,,	265	349	84		
2 inches	,,	,,	247	351	104		
,,	,,	,,	253	355	102	102·0	
,,	,,	,,	255	355	100		
,,	,,	reversed	452	350	102		
,,	,,	,,	453	350	103	102·7	1·10
,,	,,	,,	448	345	103		
,,	negative	,,	224	322	98		
,,	,,	,,	227	317	90	92·7	1·05
,,	,,	,,	227	317	90		
,,	,,	direct	460	365	95		
,,	,,	,,	459	359	100	97·7	
,,	,,	,,	460	362	98		

TABLE II.—SMALL GLOBE AND PLATE.

Length of Spark.	Charge on Globe.	Connections of Electrometer.	Deflection.	Zero.	Difference.	Mean.	Ratio of Positive to Negative.
¼ inch	negative	direct	505	367	138		
,,	,,	,,	500	370	130	134·7	
,,	,,	,,	505	369	136		
,,	,,	reversed	215	342	127		
,,	,,	,,	219	339	120	124·7	·91
,,	,,	,,	218	345	127		
,,	positive	,,	473	347	126		
,,	,,	,,	475	350	125	122·3	·92
,,	,,	,,	470	354	116		
,,	,,	direct	235	345	110		
,,	,,	,,	230	345	115	115·3	
,,	,,	,,	228	349	121		
½ inch	positive	direct	170	340	170		
,,	,,	,,	165	340	175	173·3	
,,	,,	,,	170	343	173		
,,	,,	reversed	550	360	190		
,,	,,	,,	545	365	180	186·0	·90
,,	,,	,,	545	357	188		
,,	negative	,,	125	313	188		
,,	,,	,,	119	312	193	191·7	·88
,,	,,	,,	118	312	194		
,,	,,	direct	580	368	212		
,,	,,	,,	590	379	211	211·7	
,,	,,	,,	590	378	212		
¾ inch (1)	,,	,,	610	390	220		
,,	,,	,,	613	394	219	220·7	
,,	,,	,,	613	390	223		
,,	,,	reversed	100	335	235		
,,	,,	,,	100	333	233	234·0	1·01
,,	,,	,,	98	332	234		
,, (2)	positive	,,	580	370	210		
,,	,,	,,	600	370	230		
,,	,,	,,	580	365	215	222·5	·92
,,	,,	,,	600	365	235		
,,	,,	direct	115	337	222		
,,	,,	,,	125	338	213		
,,	,,	,,	120	333	213	216·0	
1 inch (3)	,,	,,	105	335	230		
,,	,,	,,	110	330	220	224·0	
,,	,,	,,	105	327	222		
,,	,,	reversed	605	353	252		
,,	,,	,,	610	365	245	245·3	·90
,,	,,	,,	605	366	239		
,, (4)	negative	,,	65	315	250		
,,	,,	,,	65	315	250	247·7	1·11
,,	,,	,,	70	313	243		
,,	,,	direct	610	380	230		
,,	,,	,,	610	392	218	221·0	
,,	,,	,,	610	395	215		

(1.) Brushy sparks giving an incomplete discharge and preceding the white spark first observed. (2.) No brushy sparks observed before the white spark. (3.) Brushy sparks observed before the white spark. (4.) Great number of brushy sparks before the white spark.

6. Researches in Thermometry.

By Edmund J. Mills, D.Sc., F.R.S. (Communicated by Professor Crum Brown.)

(*Abstract.*)

Having had occasion in the course of my work to investigate some of the properties of the mercurial thermometer, I have obtained a series of results which are comprised in a memoir now submitted to the Society. A brief summary of these is given in the following abstract.

1. After describing a simple method of calibrating a thermometer, I give a detailed proof (following Pierre) that the calibration unit gradually undergoes a slight diminution in value. In the course of five years this may amount, in a new thermometer, to as much as ·21 per cent. The 0°–100° interval, therefore, requires periodic verification.

2. When the indicating part of a thermometer has a different temperature from the bulb, an "exposure" correction becomes necessary. If y represent the value of this correction, it is generally determined from the equation—

$$y = \cdot 000\,1545(\mathrm{T} - t)\mathrm{N}\,;$$

where ·000 1545 is the difference between the co-efficients of cubical expansion of glass and mercury, T is the reading of the thermometer, and t is the mean temperature of the exposed portion N. Experimental evidence is adduced in the memoir to show that the factor of (T–t) N is not a constant quantity, but a linear function of N. The equation thus becomes—

$$y = (\alpha + \beta\mathrm{N})(\mathrm{T} - t)\mathrm{N}\,,$$

where α and β are constants to be determined from the experiments. The values of α and β are very small, and from about 1500–2000 eye-observations were required to determine them, according to the instrument employed.

3. The gradual ascent of the zero with time can be expressed by a logarithmic curve having two terms, viz.—

$$y = \mathrm{A}\alpha^x + \mathrm{B}\beta^x\,,$$

where y is the remaining ascent, x the time, and A + B the total ascent. After two or three intervals x, the value of $B\beta^x$ is usually inconsiderable; so that the ascent may at an early period be represented by the simple expression—

$$y = A\alpha^x .$$

The probable error of a single comparison of theory with experiment is, as a mean result of eleven curves, 0°·012 C. Incidentally, it is shown that the above law is obeyed whether the thermometer is vacuous or open to the air. In a similar manner, the movements of the zero with temperature can be expressed by the equation—

$$y = A\alpha^x - B\beta^x ,$$

where x represents equal successive intervals of temperature. In this case, the value of $B\beta^x$ is always appreciable. The probable error of a single comparison of theory with experiment is, as deduced from four curves, 0°·023 C.

Under the influence of heat, the zero of a vacuous thermometer at first descends, until the heat reaches a definite degree for each instrument; the "mean degree" observed was 154°. After this, the material of the bulb becomes, I think, semi-plastic and gives way to atmospheric pressure, the zero then rising. This phenomenon continues until the mercury has a sensibly strong vapour tension, which causes enlargement of the bulb and depression of the zero. A thermometer open to the air and kept upright, should, of course, on the application of an increasing heat, exhibit nothing but depression in the zero; and this is shown to be practically the case.

4. The correction known as Poggendorff's is then alluded to, and the importance of habitually employing it is distinctly pointed out.

5. The results of compressing a thermometer's bulb show that, up to 134 atmospheres, the effect is a linear function of the pressure. The instrument is in fact an excellent pressure-gauge.

6. In the course of the memoir, an apparatus is described for comparing the mercurial with the air thermometer, and the results of the comparison are stated. Attention is drawn to a compound

bath, having a principle believed to be new, and probably available in many cases where the maintenance of an exact temperature is required.

7. Reference is lastly made to an investigation of the melting-points of certain easily accessible and purifiable chemical substances, for which the results of the present researches have been utilised. The calculations are not quite complete; but I trust to place, at an early date, these important constants in the hands of physicists. The extremely tedious work of comparing the mercurial with the air thermometer will then, for a considerable length of scale, be in the future to a great extent avoided.

BUSINESS.

The following candidates were balloted for, and declared duly elected Fellows of the Society:—Professor J. H. Scott, Otago; Mr James Graham; and Dr R. Sydney Marsden.

Monday, 17th May 1880.

DAVID MILNE HOME, Esq., Vice-President, in the Chair.

The following Communications were read:—

1. Preliminary Notice of a Method for the Quantitative Determination of Urea in the Blood. By John Haycraft, M.B., B.Sc.

The subject of these investigations, some of the results of which are before the Society to-night, was suggested to me by Professor Carl Ludwig, and was carried out in his laboratory at Leipzig, where I worked with the help of his assistant, Dr Drechsel, to whose kindness and large knowledge of the subject I was much indebted.

In this paper I shall give simply an account of the method of analysis, leaving for the future a record of the facts which it has enabled me to obtain.

The estimation of a quantity of urea in a pure form, or in a

watery solution of fair strength is not difficult; nothing, indeed, can well be easier. There are a number of methods which we might employ, and which are exact and easy of application; such are those of Liebig, of Heintz, and Ragsky, of Bunsen, of Huefner, and of Davy, all of which may be applied for its estimation in urine.

With the blood it is another question, for here this substance exists in very small quantities (3 parts in 10,000), mixed with a mass of organic matter, from which it has first to be separated in a tolerably pure form before the quantity can be ascertained. This separation from the blood is the difficult task to be overcome, the estimation of the quantity will then be easy.

The subject is one of great importance in physiological chemistry, as all will admit, for it is a key to our knowledge of the metabolism of albuminous substances in the body, nearly all the nitrogen which is excreted passing out in this form.

This importance has been thoroughly appreciated by chemists, and many men of note have turned their attention to this subject.

It was Sir Robert Christison who first gave the stimulatus to modern inquiry, for he it was who found a large amount (much above the normal) of urea in the blood of patients suffering from Bright's disease.

His method did not pretend to great chemical accuracy, for he took only the clear serum of the blood, from which he crystallised out the urea as a nitrate after concentration.

This discovery ranks with the highest that chemists have made in their investigations of the healthy and diseased tissues, and few indeed have been the facts gleaned gradually, and with difficulty in later years.

Physiologists have endeavoured, but in vain, to found an accurate analytical process for its determination in blood, in order to investigate the many changes which occur in its amount during different physiological and pathological conditions. Lecanu, Marchand, Simon, Millon, Pettenkofer, Joseph Picard, Gscheidlen, and Drechsel have all worked with this object in view.

Indeed the chemistry of blood is beset with difficulties, as all

will admit; so many nitrogenous substances exist in it; these are closely allied one to another in their chemical relations, hence their separation is very difficult; and, lastly, during the process of separation, one substance may be changed into another.

In the case of urea estimation, fresh obstacles stand in our path which it is necessary to understand, in order that their removal may be attempted. Urea not only entirely decomposes when heated over 120° C., but when a watery solution is evaporated to dryness, part of it decomposes, producing a loss which varies of course with the quantity of water and the strength of the solution. A fraction of a gramme evaporated in a litre of water loses from 3 to 4 per cent. Now it is necessary to separate the albumen of blood from the urea, which entails the addition of much fluid, which fluid has to be evaporated down when decomposition of part of the urea ensues.

A common way is to coagulate the albumen with hot alcohol when three volumes of spirit are at least required. With acidulated boiling water, six or seven volumes are necessary for the complete coagulation. Besides this, the decomposition on evaporation is much increased if other organic impurities are present in the fluid. So much so, that if ordinary defibrinated undiluted blood be evaporated in flat dishes, even with a gentle heat, not a trace of urea is to be discovered in the hard black cake which results. Nay, a large quantity of pure urea may have been previously added, the whole decomposing during the evaporation. This is also the case with the watery and alcoholic extracts of urea from blood, for these contain much extractive matter of which the urea forms but a small portion. The loss which occurs during these evaporations is far more than would occur were the urea alone present in an alcoholic or watery solution.

Another difficulty in our way is that no substance was known which might be useful in its extraction, and in which it is insoluble. It is often thought to be insoluble in sulphuric ether, but this is far from the truth; indeed it is so soluble that ether can never be used to separate, say fat from it, in an analysis which professes to be quantitative. Urea, it may be stated, is very soluble in water and alcohol, and is soluble also in chloroform and acetic ether.

My first endeavour was to find a liquid in which urea is insoluble. A number were tried, and at last a substance, petroleum naphtha (a naphtha lately imported from America, and which is sold with other naphthas under the name of benzoline), was found in which urea is quite insoluble, and by means of which it is possible to separate urea from oil when both are in alcoholic solution. This petroleum naphtha, or benzoline, as we shall afterwards see, is of great service.

An endeavour was now made to prevent or lessen the loss which occurs on evaporating a solution of urea.

It was thought that possibly the high temperature used was prejudicial, and accordingly evaporations were conducted with solutions of known strength at a temperature lower than is ordinarily used. The solutions were evaporated on a water bath in flat dishes at a temperature of from 40° to 50° C., but, unfortunately, this was of little avail, the difference in the result being but very slight.

Failure also resulted when the urea solution (not an artificial one) was evaporated at a very low temperature and with diminished atmospheric pressure. The solution was acidified with acetic acid with loss following its evaporation.

Urea forms with acids salts of definite composition, and of well-marked crystalline form.

It was thought that by changing this substance into an oxalate or nitrate these might prove more stable, and as the ordinary methods admit of their estimation in these forms, the process might succeed, or at any rate it would be easy to reduce them as a last step into urea again.

A partial success was the result of this trial; for on evaporating an alcoholic solution of oxalate of urea (0·1 gramme per litre) there was not quite 2 per cent. loss. Indeed, a certain degree of decomposition seemed inevitable, and no way was found out of the difficulty. No method which involved evaporation could be said to be perfectly accurate. It is still possible to reduce this loss by using as little fluid as possible, and to obtain the urea unmixed with other organic solids, the presence of which is so prejudicial to the accuracy of our results.

To separate the albumen from blood, either with boiling alcohol

or hot acidulated water, requires a dilution of the blood with several volumes of water as we have seen; worse than this the alcoholic or watery extract of urea thus obtained contains so many extractives that the urea passes through several processes before it is in a fit condition to estimate, loss occurring during each process.

Can we lessen or altogether prevent this loss is the question before us?

Hearing that tungstic acid does not precipitate urea, and knowing also that it precipitates most organic substances, I tried to separate the urea by this agent.

Diluting with only $\frac{2}{3}$ vols. it was possible very completely to separate the albumens as a fine granular mass by the addition of tungstate of soda and acetic acid.

The after process, however, was so complicated (the urea had to be separated from the tungstic acid, and acetate of soda, and many extractives) that the loss was as great as before.

The separation by dialysis had long been thought of, for it was naturally suggested by the epithelium of the kidneys, by which urea is separated during life from the blood.

It had not been tried, however, as it promised to be tedious. Hearing, however, from Professor Browne that by placing blood into a dialyser with alcohol in the outer vessel it became quite hard and dry in a few hours, encouragement was given to a trial of this method.

Urea being very soluble both in alcohol and water, it would probably pass out with the fluid parts of the blood into the alcohol; a trial proved this to be the case, and a few experiments sufficed to found the method which will now be described.

80 c.c. or 100 c.c. of defibrinated blood are placed within a dialyser so as to form a thin layer on the parchment paper. The dialysers that I use are of glass, the inner vessel having a diameter of 8 to 9 inches, the outer one-half or three quarters of an inch more. The blood must not form a layer more than about 3 mm. thick, otherwise the lowest stratum will become dry and impervious while the upper will still remain fluid.

One hundred c.c. of alcohol are poured into the outer vessel, and the whole is covered. In from four to eight hours the alcohol

in the outer vessel has risen, being increased in volume by the fluid parts of the blood, which now forms a dark tough cake within the dialyser, which, as a rule, sticks to the parchment paper. Of course this contains still much urea, and it must be washed. It is not sufficient to pour fresh water upon it; it must be detached from the parchment paper and bruised with fresh water in a mortar. For this purpose the parchment paper with the blood attached is brought into a flat dish gently heated over a water bath, when the blood may be detached with the help of a little warm water. Fresh parchment paper is now placed upon the ring of the dialyser, and the bruised mass brought into it and placed again in the alcohol. The blood soon becomes dry again from abstraction of water, and the alcohol is now evaporated after acidification with oxalic acid.

The blood in the dialyser is now warmed to drive off the little alcohol that is mixed with it, and is replaced in the dialyser with some more water, with which it can now be mixed with a curved spatula; for now it forms a finely granular mass, and does not cling to the parchment paper. When this is dry it is washed in the same way for the last time.

The alcohol here is also evaporated and the residue extracted with a little alcohol which is evaporated. An all-important point is this, that mixed with the urea, which may even now be seen on the dish, there is but little organic matter. Most of this can now be removed by washing the residue with petroleum naphtha. It is then extracted with acetic ether and the urea estimated.

I accomplished this in Germany by a method introduced by Bunsen; the urea is heated with an ammoniacal solution of chloride of barium in sealed tubes at 200° C.; decomposition of the urea into ammonia and carbonic acid occurs, and the amount of this is determined by weighing the carbonate of barium which is formed.

Lately I have used a shorter and more suitable method introduced by Huefner. The urea is mixed with a solution of hypobromide of soda, when carbonic acid and nitrogen are formed. The latter is collected, and the quantity calculated where the amount of urea may be inferred. There is loss with this process of about

7 per cent., for by calculation 1 gramme of urea should yield 300 c.c. of nitrogen at 0° C. and with 760 mm. of pressure.

It only yields, however, in practice, 340 c.c., the deficit is, however, very constant.

By adopting a modification of this, for which I am indebted to Professor A. Gamgee, almost all the nitrogen is given off. This is the addition of ordinary cane sugar syrup, when you obtain 363·4 c.c. of nitrogen.

This process was, of course, tested with great care in order to find out whether it was sufficiently accurate. Two equal portions of the same blood were taken, and to one was added a known quantity of pure dry urea. If the method was perfectly accurate the results of the two fluids when analysed should show a difference equal to the quantity of urea added.

That this was not the case is evident; for although the method approaches perfection, yet it is far from being perfect, and it would only lead to disappointment if its faults were passed lightly over.

Solutions of urea have still to be evaporated although in a comparatively pure form and with far less fluid. This, as we have shown, means loss.

This loss, however, is brought to a minimum, and is as a result of my test analyses not more than 7 per cent. or 8 per cent. Now, as part of this loss is constant, and as urea is actually obtained and estimated as such, the fallacy is less than these numbers would indicate.

That this method will ere long be supplanted by another and a better one I have no doubt, and, indeed, while working at its application to the investigation of physiological conditions, I am at the same time continually trying to improve the method. So exact, however, is it that one may always readily obtain a demonstration of urea from so small a quantity of blood as 10 c.c. I have here a specimen of urea in a very pure form from 15 c.c.: this has been frequently purified, and is mixed now with little else; one might almost estimate its quantity by weight. I may state that in blood far more urea is present than is ordinarily imagined. I have obtained 56 parts per 100,000, which is a very large quantity. The normal is between 40 and 30 part 100,000.

In summing up, this method is, I believe, reliable, but not sufficiently accurate to investigate minute changes.

I have already gleaned several important facts from its application, which I hope will form the subject of another paper. As a footnote I may add that the alcohol in the dialyser is green, and on evaporation green flakes are deposited; these are in many respects like bile pigment, but I have not worked sufficiently at the subject to give a dogmatic statement. It is stated that bile pigments do not exist in the blood. I shall not here enter upon the subject, as it would be premature; the point is simply mentioned, as, if my experiments are repeated, surprise might be expressed that I had not observed this fact.

A remark may be made here upon the separation of albumen by acidulated boiling water. I have reason to believe that this is a method which is thoroughly bad, although it is one which is constantly had recourse to in two or three branches of blood analysis. The coarseness of the coagulum varies with the temperature of the water.

If blood be poured into water which is thoroughly boiling the albumen collects in masses the size of a small bean, or sometimes in much larger pieces. If the water be not so hot, then the coagulation is finer.

Now, it is extremely difficult to wash out extractives from these albuminous masses, the difficulty increasing with their size. Hence your results may vary not with the amount of substance present in the blood, but with the coarseness of the coagulum, which is not desirable. I have tested this in the case of urea, and my friend Dr Bleile confirms the fact in the case also of sugar.

2. On the Phenomena of Variation and Cell-Multiplication in a species of Enteromorpha. By Patrick Geddes. Communicated by Professor A. Dickson.

3. On the Accurate Measurement of High Pressures. By Professor Tait.

In the course of an examination of some of the "Challenger" deep-sea thermometers, I have recently had occasion for measurements, accurate to one or two per cent., of pressures such as five or six tons weight per square inch. The ordinary gauges showed themselves to be quite untrustworthy, and it was necessary to devise some plan of whose accuracy the experimenter can feel assured. The following process has proved completely successful, and is capable of any desired degree of accuracy.

Simple methods based on the compression of gases, such as air or nitrogen, are of the highest value wherever they can be adopted; for the law of compression of these bodies is known with great accuracy (at least for one definite temperature) from the measurements recently made by Amagat, in which the pressures were directly reckoned in terms of a column of mercury. A simple form of gauge, in which the column of mercury compressing the gas into a small bulb at the extremity is made to break off at a constriction in the connecting tube, enabling us (by weighing the mercury forced over into the bulb) to measure the compression very accurately, suffices amply for all pressures up to a ton weight per square inch, or even farther.

But this instrument becomes rapidly less and less sensitive at higher pressures; so that though the law of compression for a considerably extended range is now known, for pressures above a ton something else is required.

Hooke's Law now comes to our assistance. An instrument resembling a thermometer in form (but with a tube of much larger section compared with the capacity of the bulb than is usual in mercury thermometers) supplies the next step. It is filled with mercury (because of the small expansibility of that liquid), and is thus practically unaffected by small changes of temperature. Over the mercury in the stem is a long column of alcohol in which the index moves, and the rest of the tube contains alcohol vapour only. The bulb is made cylindrical for several reasons; the chief being to secure uniformity of thickness, which is practically unattainable

(or at least unverifiable) in a sphere. By properly choosing the *thickness* of the cylinder in proportion to its bore, the sensitiveness of this gauge may be made as great or as small as we please. And, by having two or more, with bulbs of nearly the same *internal* dimensions, but differing considerably from one another in thickness of the cylindrical walls, a very important advantage is secured. For, under the same pressure, the maximum amounts of distortion of the glass are greater in the thinner bulbs, and thus these begin to deviate from Hooke's Law at pressures under which the thicker ones are still following it accurately. Thus, by comparison, we can easily find through what portion of its range each instrument gives effects strictly proportional to the pressure. The thinnest of these is to be graduated by comparison with the nitrogen gauge.

When this method has to be extended to pressures such as would crush glass, recourse must be had to steel, and a series of instruments with different thicknesses of this material is to be prepared. I do not yet know whether it may be found practicable to furnish these steel bulbs with thick glass tubes of small bore—probably we may succeed, if the steel be made to project into the glass. But if not, it is easy to construct them entirely of steel, so as to act on the principle of the "weight-thermometer." Anyhow, they can be graduated accurately from one another, each from a thinner one; until we come to the thinnest, which is to be exactly graduated by comparison with one of the thicker of the glass instruments. We have thus a series of gauges, each of any desired sensitiveness, capable of reading accurately pressures up to those for which steel at the interior of a thick tube ceases to follow Hooke's Law.

To illustrate this process, and to show what amount of sensitiveness is to be expected from an instrument of known dimensions, I append an approximate solution of the problem of the compression of a cylindrical tube with rounded ends. The exact solution would be very difficult to obtain, and would certainly not repay the trouble of seeking it. I content myself, therefore, with the assumption that all transverse sections are similarly distorted, which, of course, involves their continuing to be transverse sections.

Let ξ denote the displacement of a transverse section originally distant x from one end, and let ρ be the change of r the original distance of any point of the section from the axis. Then, as it is

obvious that the principal tractions are along a radius, parallel to the axis, and in a direction perpendicular to each of these, we have at once (Thomson and Tait, *Nat. Phil.* §§ 682, 683)

$$\frac{d\rho}{dr} = et_1 - ft_2 - ft_3 ,$$

$$\frac{\rho}{r} = -ft_1 + et_2 - ft_3 ,$$

$$\frac{d\xi}{dx} = -ft_1 - ft_2 + et_3 ,$$

where $$e = \frac{1}{3n} + \frac{1}{9k}, \qquad f = \frac{1}{6n} - \frac{1}{9k}.$$

Here $\frac{1}{k}$ is the compressibility, and n the rigidity.

In addition we have for the equilibrium of an element bounded by concentric cylinders, planes through the axis, and planes perpendicular to it,

$$t_2 = \frac{d}{dr}(rt_1) ;$$

and the approximate assumption above gives

$$\frac{d\xi}{dx} = \text{constant}.$$

From these five equations t_1, t_2, t_3, ρ, and ξ are to be found.

They show that t_3 is constant, and its value must therefore be

$$-\Pi\frac{a_1^2}{a_1^2 - a_0^2}.$$

With the surface conditions,

$$t_1 = -\Pi \text{ when } r = a_1 ,$$

$$t_1 = 0 \quad ,, \quad r = a_0 ,$$

we determine the arbitary constants, and it is easy to see that

$$\frac{\rho}{r} = -\Pi\frac{a_1^2}{a_1^2 - a_0^2}\left(e - 2f + \frac{a_0^2}{r^2}(e+f)\right)$$

$$\frac{d\xi}{dx} = -\Pi\frac{a_1^2}{a_1^2 - a_0^2}(e - 2f).$$

Thus the diminution per unit volume of the interior of the cylinder is

$$-2\left(\frac{\rho}{r}\right)_{a_0}-\frac{d\xi}{dx}=\Pi\frac{a_1^2}{a_1^2-a_0^2}(5e-4f)=\Pi\frac{a_1^2}{a_1^2-a_0^2}\left(\frac{1}{n}+\frac{1}{k}\right).$$

When Π is a ton-weight per square inch, the value of the quantity

$$\Pi\left(\frac{1}{n}+\frac{1}{k}\right)$$

is somewhere about $\frac{1}{1000}$ for ordinary specimens of flint glass, and about $\frac{1}{4000}$ for steel.

It is obvious from the formulæ above, from which we have

$$\frac{dp}{dr}=-\Pi\frac{a_1^2}{a_1^2-a_0^2}\left(e-2f-\frac{a_0^2}{r^2}(e+f)\right)$$

that the greatest of the three compressions is that perpendicular to planes through the axis, while the least is radial. The former has its maximum

$$\frac{\Pi a_1^2}{a_1^2-a_0^2}(2e-f)=\frac{\Pi a_1^2}{a_1^2-a_0^2}\left(\frac{1}{2n}+\frac{1}{3k}\right)$$

at the inner surface where, therefore, the tube will first yield to crushing; the latter has its maximum at the outside. Their sum is constant.

If we compare two tubes with the same internal bore, 5^{mm}, but one two millimetres thick while the other is only half a millimetre, the maximum distortions under the same pressure are as $\frac{81}{56}$ to $\frac{36}{11}$ or 4:9 nearly.

When the pressure is internal we have

$$\frac{\rho}{r}=\frac{\Pi a_0^2}{a_1^2-a_0^2}\left(\frac{1}{3k}+\frac{a_1^2}{r^2}\frac{1}{2n}\right),\quad \frac{d\xi}{dx}=\frac{\Pi a_0^2}{a_1^2-a_0^2}\frac{1}{3k};$$

and the increase per unit volume of the interior is

$$\frac{\Pi a_0^2}{a_1^2-a_0^2}\left(\frac{1}{k}+\frac{a_1^2}{a_0^2}\frac{1}{n}\right).$$

In very thick tubes of narrow bore this is roughly $\frac{\Pi}{n}$, the value of which in glass is about $\frac{1}{1600}$ only for one ton pressure.

When the pressure is the same outside and inside the cylinder, we have

$$\frac{\rho}{r} = -\frac{\Pi}{3k}, \qquad \frac{d\xi}{dx} = -\frac{\Pi}{3k},$$

and the diminution per unit volume of the interior is, as in Örsted's experiment,

$$\frac{\Pi}{k}.$$

For a spherical bulb the equations are reduced to

$$\frac{d\rho}{dr} = et_1 - 2ft_2,$$

$$\frac{\rho}{r} = -ft_1 + (e-f)t_2,$$

$$2rt_2 = \frac{d}{dr}(r^2 t_1).$$

and we have for external pressure Π,

$$\frac{\rho}{r} = -\Pi \frac{a_1^3}{a_1^3 - a_0^3}\left(\frac{1}{3k} + \frac{a_0^3}{r^3}\frac{1}{4n}\right).$$

As an external indication of the pressure (to guard against carrying it too far), a cylindrical steel bulb, screwed inside the compression apparatus, and filled with mercury, is used. Its indications are read by a glass tube inserted in its neck, which opens outwards.

I hope by means of the apparatus I have described to be able to measure approximately the volumes of gases and liquids at pressures amounting to 15 or perhaps 20 tons on the square inch. The only difficulty in the case is that at these high pressures the compressibility of such bodies is probably of the same order of small quantities as that of solids.

[*Added during printing.*—To diminish the effect of temperature on these instruments, the interior of the cylindrical bulb is now *nearly filled* by a glass tube sealed at both ends. Thus the quantity of liquid in the bulb is not much greater than that in the stem.]

4. Sixth Report of the Boulder Committee.

(Plates XVII., XVIII., XIX.)

The materials for this Report have been obtained from the Convener, Professor Forster Heddle of St Andrews University, William Jolly, Esq., H.M. Inspector of Schools, Inverness, and William Wallace, Esq., High School, Inverness.

To make the descriptions of the boulders more intelligible, it has been found necessary, as in former Reports, to annex a few diagrams, which will be found at the close of the Report.

I. *NOTES BY CONVENER.*

Argyleshire.

1. In consequence of information in the schedule issued by the Committee, and filled up by Mr Montgomery, schoolmaster at *Southend*, near Campbelton, I went to Southend, and had pointed out by Mr Montgomery boulders at several places there.

Mr Montgomery considered these boulders to be granite. If granite, they were different from any I had ever before met with. They were certainly an igneous or primitive rock of some kind, and composed of different ingredients, the chemical nature of which I am unable to state. There appeared to be crystals of white felspar. I could detect no mica. The general mass was a whitish-grey colour.

These boulders were pointed out to me at many places. I saw two in the River Conn (about half a ton in weight), one on Pennyserach farm (3 or 4 tons), another on the adjoining farm of Brunerican ($1\frac{1}{2}$ ton), another at Macherioch (about half a ton in weight). I heard of many more lying on the sea-shore adjoining these farms.

I was assured by Mr Montgomery, and by the tenants of these respective farms, that there was no rock in the south end of Cantyre similar to that of these boulders.

Along the east coast between Campbelton and Southend, a distance of 8 or 10 miles, I descried many boulders of the same nature; and in a gravel pit near Campbelton gas-work, I saw the fragments of another, which had weighed probably 3 or 4 tons.

In this same gravel pit I found a boulder of grey porphyry, con-

taining crystals of white felspar, somewhat similar to the Southend boulders. The gas manager informed me that he believed there was rock of the same nature a few miles to the north.

In the valley of Brackerie I found rock of a crystalline nature somewhat similar in composition to the boulders above described; and at a place in the same valley, called Collielangart, I saw a monumental pillar, about 8 feet high, similar in composition, said to have been obtained from Glenlissa, a place about 3 miles to the N.W. of Campbelton. I was informed also that boulders of this same rock, weighing 2 or 3 tons, had lately been observed in a recent cutting into boulder-clay to the north of Campbelton.

Professor Nicol, in a short account of the Geology of Cantyre in the "London Geological Society's Journal" for 1852 (vol. viii. p. 421), refers to the boulders at and near Southend. He describes them as *white granite*, and as resembling a granite in Arran, from which, therefore, he supposed these Southend boulders had somehow been transported. Professor Nicol takes notice of several striated rocks on the east coast of Cantyre, one of which showed a direction of E. 10° N. by compass, which he remarks is nearly parallel to the line of coast, and in the direction of Arran, 25 miles distant.*

There was only one spot where I found a smoothed rock, viz., about a mile to east of Campbelton. It sloped down to N.N.W. at an angle of 40°. There were no striæ.

I offer no positive opinion regarding the position of the parent rock from which these *white granite* boulders came. It is pretty clear, however, that those at Southend must have travelled from the north, and many of them there are lying on the Old Red Sandstone strata which fringe the south-west coast of Cantyre.

Another part of Cantyre visited by me was the district between Campbelton and the west coast at *Drumlenbie and Balahunty*.

Near *Kilhenzie*, a few miles west of Campbelton, there are hills reaching to a height of from 500 to 600 feet, well covered with detritus; and on their western slopes there are numerous boulders of

* With reference to Professor Nicol's view that the white granite boulders seen on the east and south coast of Cantyre came from parent rocks in Arran, it is right to notice that the late Rev. Mr M'Bride of Rothesay, who was a good geologist, and well acquainted with the rocks of the West Highlands, suggested a more northern source (Bryce "On Arran," 4th edition, p. 337).

gneiss and mica schist. I measured several, the largest contains about 150 tons.

The Old Red Sandstone formation occupies the west coast for some miles. It is well covered by detritus, and on the detritus, especially when it slopes to the west, there are many boulders of granite and gneiss, which from their position appear to have come from the N.W.

Fig. 1, plate XVII., represents a bank of gravel, at a height of about 50 feet above the sea, and sloping to the sea in a N.N.W. direction at an angle of about 25°, well covered with gneiss boulders, of which three are represented. There was no rocky cliff from which they could have fallen. They were true erratics. Thick turf had formed on the bank, which partly covered the boulders.

Fig. 2, plate XVII., represents, near the above spot, another boulder of gneiss, at a height of about 40 feet above the sea, lying on a mass of reddish coloured mica schist, blocked at its south end. Its longer axis lay N. by E. and S. by W. It had apparently come from the north, and been stopped in its further progress by the rocks at its south end.

On the shore, I found a boulder partially in a fissure which cuts through the mica schist strata, here forming tablets or sheets nearly horizontal. Figs. 3 and 4, plate XVII., represent a fissure running N.W. and S.E., about 6 feet wide. The boulder, very hard gneiss, $10 \times 5 \times 4$ feet, was sticking in this fissure. Fig. 3 represents the fissure running N.W. and S.E., and the upper part of the boulder projecting above it. Fig. 4 represents the interior of the fissure partially filled with pebbles, and the boulder resting partly on them and partly on the S.W. wall of the fissure, whilst the other end of the boulder, outside and above, was resting on the N.E. edge. The boulder had clearly been pushed from the northward (from B in figs. 3 and 4), and on reaching the fissure had partially fallen into it, and become jammed there.

If the idea of a sea-current from the north, with ice floating in it, be entertained, there seems to be no difficulty in explaining the above facts. The weight of the boulder was about 15 tons.

On the same part of the shore, there were many other hard gneiss boulders. The largest measured was $12 \times 6 \times 6$ feet, its longer axis pointing north and south.

Specimens of these boulders, found by me in Cantyre, I submitted to Professor Heddle of St Andrews University, so well known for his acquaintance with the igneous rocks of Scotland, and their mineralogical composition. He has kindly supplied the following notes:—

(1) Most of the Southend boulders, and those along the east coast between Campbelton and Southend, are identical in composition with one variety of the coarse porphyritic rock of Davar Island, situated at the mouth of Campbelton Bay.

(2) One specimen is a small-grained white granite, which I think I have seen somewhere in Arran.

(3) One specimen from the west coast is a coarse grey granite, identical in appearance with the granite of the Mourne Mountains in the N.E. of Ireland. I observe in this specimen two crystals of topaz. This granite, from containing also crystals of albite and of a lithian mica, should be easily recognised.

2. *Loch Lomond.*—On the west side of the lake, near Arden, a lateral valley runs up towards the west. There is a horizontal terrace in this valley about 70 feet above the lake, bounded by a steep bank, showing that at one time the lake had filled the valley up to that height. On this flat lie a number of quartz, granite, and mica schist boulders, which most probably all came from the westward, as the rocks in the valley are Old Red Sandstone. The head of the valley reaches to about 150 feet above the sea. The land then slopes down westward towards the sea in Loch Long. If these boulders were floated from the westward, it must have been when the sea was at a greatly higher level than 150 feet. The largest of these boulders, a mica schist, I found to be $5 \times 3 \times 3$ feet, with its longer axis lying E. and W., and its sharpest end towards the west.

On the east bank of the loch, nearly opposite to Arden, on the farm of Over Balloch, and at a height of about 337 feet above the sea, I found a grey granite boulder, $5 \times 4 \times 4$ feet, much rounded. Its longer axis lay in like manner E. and W. It was on a bed of boulder-clay. It most probably had come from the west or north-west, crossing therefore the valley now occupied by Loch Lomond.

3. *Loch Long and Gareloch.*—On the ridge between Gareloch and Loch Long I found several boulders. At a height of 160 feet above

the sea, there is one of mica slate, 11 × 6 × 6 feet, lying on rocks of clay slate. Its longer axis is N. by E. and S. by W., its sharpest end being to the north. The axis was parallel to the valley of Loch Long. Its south end was pressing against a knoll of gravel as shown on fig. 7, plate XIX., which seemed to have intercepted its farther progress to the south. This boulder had apparently come down Loch Long, though whether floated by ice or carried by a glacier, is a question. But the knoll of water-borne gravel at its south end, favours the former theory.

Another boulder on this same ridge, 8 × 6 × 5 feet, occurs at about 360 feet above the sea, also blocked at its south end by a rocky knoll.

In the Gareloch, on the east side, a little below Shandon, on the beach, a gneiss boulder occurs, 18 × 15 × 12 feet, with its sharp end pointing N.N.W. The boulder on that side presented a very smooth surface. Every other side was rough.

The foregoing boulders as regards *position*, may all be accounted for by the supposition of the transporting agent having passed through the valleys in which they are situated, in a southerly direction.

The boulders in the Gareloch and Loch Long were reported on by the late Charles Maclaren, and the opinion which he formed was that they had been brought to their present position by floating ice.

It appears that the late Sir Roderick Murchison visited this district, and gave an opinion against the theory of glaciers as applicable here.—(Chambers in "Edinburgh New. Phil. Journ.," vol. lv.)

4. *Loch Fyne.*—I was invited by Mr M'Killop, schoolmaster at *Loch Gair*, situated about 7 or 8 miles west of Inveraray, to inspect some large boulders in that district.

The first block seen was situated about 3 miles to the north of Loch Fyne, surrounded by hills, most of them covered by drift. It was 23 × 17 × 12 feet, its longer axis lying N.N.E. and S.S.W. It was resting on a knoll of gravel, and at some distance from any hills. It was clearly an erratic,—a coarse gneiss. At first I was puzzled to account for its position being so exactly on the apex of the gravel knoll. It struck me eventually, that its great size and weight had been the means of protecting, by covering the knoll on which it originally had been dropped. The denuding agencies which could loosen and sweep away the gravel and sand in the surrounding parts of the

valley, probably did not move the boulder, and so would leave it in its original position or nearly so.

I proceeded next towards *Loch Glashan*, and was rather surprised to see the hills on its south side, which sloped down towards the N.E., well-covered with boulders, and also striated rocks, facing N.E. and N.N.E. In fig. 5, plate XVII., there is a view of one of those boulders, $8 \times 4 \times 3$ feet, on Knock farm, resting on a smoothed rock, dipping N.N.E., at an angle of about 30°, and at a height of about 400 feet above the sea. At this place, looking towards the N.N.E., there seemed to be a sort of low level district for some miles, with high hills on each side. On examining the map, I found that Loch Awe and Loch Etive were in that direction.

5. A few weeks after being at Loch Gair, I visited *Loch Awe*, and remained for a few days at *Port Sonnachan*, situated on the south bank of the lake.

On inquiring of the innkeeper, I was informed of a remarkable boulder situated among the hills to the south, and distant 3 or 4 miles. Having obtained the services of a shepherd as guide, I proceeded on foot across the moors, and came to a high corry, with a ridge in the middle, on which ridge the boulder stands at a height of 1026 feet above the sea. The boulder is so distinguishable from every other in the district, that the corry takes its name from it, viz., *Corry na clach.*

Fig. 7, plate XVII., gives a distant view of the boulder among the hills to the south of Loch Awe. Fig. 8 shows its position on the ridge where it stands. This ridge is narrow and has steep sides, so steep that they can be climbed with difficulty. The side facing the south is about 80 feet, the side facing the north is about 50 feet, in height, above the level ground adjoining.

The ridge, shown in figs. 7 and 8, is composed entirely of a soft arenaceous mica schist, in thin slaty strata, which stand up vertically, and form a table about 3 feet above the ridge, as shown in fig. 9. This table, on which the boulder sits, is about 5 feet square. The surface of the table slopes down to W. by S. at an angle of 22°. The boulder on this table of rock occupies a most precarious position. Stooping below the boulder, to examine the strata forming the table, I saw daylight across, under the boulder, and observed

that it touched the rocky table at three points, each point of contact consisting of a few square inches.

The boulder was 13 feet long, 12 feet wide, and about 6 feet high. Its longer axis lay across the ridge, viz., about N. and S. Neither the nature of the rock composing the boulder, nor its own position, gave any indication of the direction from which it had come. It was a hard compact gneiss, the rock which prevails in most of the hills of the district on all sides. One feature in the position of the boulder offered a suggestion, though slight, as to the direction of its transport. If the rocky table on which it lay was sloping as now (at an angle of 22° to the west) when the boulder landed on the table, it is probable that it must have come from a westerly rather than from any other point. If it came from an easterly direction, it would, by its own weight when still in motion, have slid off the table altogether. But the assumption that the table on which it rests was originally sloping as now, may not be correct. On this ridge denudation may have changed the surface—except where protected by the boulder. Moreover, it is possible that the boulder itself, by the mere action of the wind upon it, might cause it to move on and abrade the rock. The space between it and the rock may also have been acted on by frost. Certain it is, that at present the stability of the boulder is most precarious. With a lever, I could easily have moved the boulder off its site. The innkeeper at Port Sonnachan informed me that there had actually been a proposal by some travellers staying at his inn, to perform this exploit, and that he had prevented it.

I am unable to explain how the boulder could have got on the apex of the hill, except on the supposition that a sheet of thick ice, strong enough to float the boulder, may have stranded on the hill; and that when it melted, the boulder might have subsided on the part where the ice had stuck.

6. Having asked my guide, whether there were any other large boulders in the neighbourhood, I was conducted by him to the side of a hill, about $\frac{1}{4}$ of a mile to the eastward, well-covered with boulders. The height above the sea was about 900 feet. I was rather surprised to find the boulders here in such positions as to indicate that they had come from N.N.E. The largest measured $18 \times 10 \times 10$ feet, and its longer axis lay N. and S. I observed that most of the other boulders lay in a similar position. The rocks presented smoothings

which faced N.N.E., being the direction in which Loch Etive lies.

I remembered that in walking up from Loch Awe on this occasion, I had seen several smoothed rocks with striæ running much in the same direction, but I omitted to take the exact bearings.

I felt surprised at this direction, as, when last year in the Hebrides and the west coast of Argyleshire, I had been accustomed to see that N.W. was the usual direction both of boulder transport and of rock striations.

7. This N.N.E. direction of transport appears, however, to characterise all the boulders and the rock striation at the *Gareloch*, *Loch Gair*, and *Loch Awe.* It will be observed that these places form a band or line across the country about N.N.E. and S.S.W. It is no doubt premature to theorise on so small a number of facts recorded in these notes. But they seem to suggest that in this district there may have been a current of floating ice, moving in a S.S.W. direction, dropping boulders where the ice which bore them was stranded or obstructed.

Is it not probable that, when the Highlands of Scotland were covered by the sea, up to a height of say 2000 feet, and when they presented an archipelago of islands, there may have been currents moving in different directions, and these directions changing as the sea fell from one level to another?

The valley through which the Highland Railway passes, between *Killin and Dalmally*, presents, on the sides facing and sloping down to the north, many examples of large boulders and striated rocks, which, even from a railway carriage, are seen to be well-deserving of special investigation. Thus at *Luib* station, and for some miles both to the east and west of it, there are numerous large boulders resting on the hill sides sloping down to the north; as also great masses of boulder-clay and water-borne gravel, with huge boulders, and occasionally under these beds, surfaces of rock, well smoothed and striated. A special examination of this district would be rewarded by many important discoveries. Similar features occur at and near *Crianlarich* and *Tyndrum.* But at Tyndrum, while there are knolls and escars of gravel, so numerous indeed that they have given a name to the place (in Gaelic),* there is a sudden

* *Tigh*, dwelling; *Drum*, ridge or back.

and remarkable cessation of boulders. This absence of boulders continues west of Tyndrum till about 2 or 3 miles east of Dalmally, when they again begin to make their appearance, and they are very numerous on the hills there facing the N.W.

May the reason of this be, that at or near Tyndrum there is the valley (traversed by the high road) running in a N.N.W. direction between high mountains, passing through Glencoe, whilst near Dalmally there is a similar opening towards the sea by Loch Awe and Loch Etive. When the sea stood at say 2000 feet above its present level, currents may have flowed through both of the openings just described, but not over the high ground between Dalmally and Tyndrum, the land there being so high that it may have prevented a current. It will be remembered that in the Committee's Fifth Report facts were stated, which seemed to show that in Glencoe a current had passed up the glen carrying boulders towards the S.E.

The current which passed through what is now Loch Etive and across Loch Awe, towards the S.S.W., may have continued till the sea sank below the level of the hills lying in that quarter. Along the banks of Loch Awe there are sea-terraces at a height of at least 200 feet above the present sea-level; and in the narrow pass at the south end of the loch, near Ford, there are indications of a current which flowed through it from the north.

Whilst on the subject of Loch Awe, I may notice a boulder on the south bank of the loch, at a place called Kaim, about 10 miles west of Port Sonnachan. The boulder is of mica schist, and is $24 \times 11 \times 9$ feet. Its longer axis is N.W. by N. It rests on a knoll of gravel which is about 20 feet above the adjoining meadow. This meadow is surrounded by hills (from 400 to 600 feet in height) on all sides but one, where there is an opening due west from the boulder, and by this opening the boulder may have entered the *cul de sac* where it lies, though, if brought when the sea was 400 to 600 feet higher than now, it may have come from any other direction.

The hills to the south of this meadow are, on their sides sloping down to the north, well-covered by boulders; and they apparently had come from some northerly point.

Along the south bank of Loch Awe, between Port Sonnachan

and Kaim, striated rocks occur, almost all of which present surfaces towards the N.W. Fig. 8, plate XIX., gives an example of one of these rocks. It slopes down N. by W. at an angle from 60° to 70°. The striæ upon it run E. and W., dipping down east at an angle of 20°. In one part of the surface there is a hollow, A, over which the striating agent has passed without marking its sides. The striating agent had moved from the west, as the striæ were deepest at their west ends.

8. *Ardrishaig.*—This place is on Loch Gilp, a small branch from Loch Fyne. There are hills here on the west side of Loch Gilp which rise to a height of from 700 to 800 feet. Fig. 1, plate XVIII., gives a bird's-eye sketch of the loch and these hills. No. 1 is the town of Ardrishaig. No. 2 is Loch Gilphead. A lateral valley comes down from the S.W. marked AB.

At *a* and *b* many of the rocks (a sort of clay slate) are (at a height of 420 feet above the sea) well smoothed, the smooth faces being parallel with the axis of the loch, which here runs about N. and S. The smoothing had evidently come from the north. On reaching *cd*, at a height of 600 feet above the sea, the smoothed rocks were more abundant, and evidently from the same direction.

A Boulder of dark-coloured granite, $12 \times 6 \times 4$ feet, was found between *ab* and *cd*, whose position implied transport from the north.

At a height of from 500 to 600 feet between *a* and *c* there are numerous whinstone knolls, sloping down towards the north as indicated on fig. 2, plate XVIII., beautifully smoothed and polished on the north sides, but rough and precipitous on the south sides. On some of them a few boulders were lying, evidently intercepted in their farther progress southward by these knolls, because on the south side there were numbers of boulders which, having been pushed over, had remained there, as shown in the figure.

At one place indicated on the sketch, fig. 1, plate XVIII., by the letter *e*, a smoothed rock was met with sloping down S.E. by S. at an angle of 35°. Two sets of striæ were on it, one running N.W. by W., the other running N.E. and S.W.

This spot was very near the corner where the lateral valley AB joins the main valley of Loch Gilp. The two sets of striæ indicated of course two several currents, one apparently parallel with the axis

of the lateral valley AB, and the other nearly parallel with Loch Gilp valley.

At the mouth of this lateral valley, at *f* on the sketch, a number of boulders were found at a height of about 670 feet above the sea, lying on the hill slopes facing the N.W. Some of these were in such positions as showed transport from the N.W. One example is given in fig. 3, plate XVIII., where a boulder A, 4 feet high by $2\frac{1}{2}$ feet wide, was resting against the N.W. sides of rocks B, on their W.N.W. sides.

On proceeding to the head of this lateral valley, about 1 mile distant, and at a height of about 795 feet above the sea, I found numerous boulders, many of large size, resting chiefly on rocks and hill slopes, facing W.N.W., and with their longer axis lying much in the same direction. One of these measured $12 \times 6 \times 5$ feet.

There were several smoothed but no striated rocks. At one place, however, at a height of 650 feet above the sea, I found a mass of softish Silurian rocks traversed by a hard quartz vein about 2 inches thick, standing up above the Silurians, as shown by A in fig. 4, plate XVIII. This vein had been evidently ground down by something which had passed over it from W.N.W. The quartz retained a beautiful polish, but the Silurian rock, though it had once presented a smooth flat surface, had become rough by atmospheric action. Being softer than the quartz vein, it had been ground down more effectually by the agent which had passed over.

9. *Loch Killesport.*—Having been informed by Mr J. F. Campbell of Islay (author of "Frost and Fire") that the largest boulder he had seen in Scotland was on the south side of *Loch Killesport*, near Ormsary House, I went there in company with Mr Alexander of Lochgilphead, who was so obliging as to undertake to be guide. His local knowledge was of much service to me.

We went first to some boulders a little beyond Ormsary House on the sea-shore. One (of gneiss) had a girth of 65 feet and a height of 16 feet. It was tolerably well rounded, its sharpest end pointing N.W. Another measured $17 \times 13 \times 5$ feet, its longer axis lying N.W. by W. Another measured $24 \times 12 \times 5$ feet, its sharpest end pointing W.S.W.

The largest boulder was situated about $\frac{1}{4}$ of a mile east of Ormsary House, on the south side of the coast road, adjoining a ruinous

cottage. The boulder was in two pieces, having evidently been broken by some natural agency. Before it broke, it must have measured in length 52 feet, in width 36 feet, and in height 20 feet, containing 1387 cubic yards or about 2770 tons.* It was extremely angular in shape. Its narrowest end was to the west. This immense boulder was at the foot of what was evidently an old sea-bank, whose base is about 40 feet above the sea at high water. The bank is about 35 feet in height, and consists chiefly of gravel and boulders.

Fig. 5 on plate XVIII. is intended as a sketch from memory of this spot, AAA being the old sea-bank, B the large boulder, and CD the high road along the south shore of Loch Killesport.

Along the line of this old sea-bank, there were great numbers of boulders above it and at its base. The measurements of a few may be given. A boulder at base of the bank, measured 23 × 14, lying E. and W.; another measuring 20 × 12 × 10 feet, lay on the slope of the gravel bank, which here faces W.S.W.

Another boulder on top of bank measured 24 × 16 × 13 feet; and another 18 × 10 × 8 feet, lying on gravel with its sharp end to the W.N.W. There were about twenty more, smaller than these, scattered on the fields above the old sea-bank.

At a little distance from the top of the bank, I found a rock of mica schist well smoothed, with striæ running E. by N. and W. by S.

Near the village of Ballibayach, in a field to the north-east, there is a gneiss boulder, 33 × 18 × 12 feet, resting on a smoothed rock of mica slate, which slopes down towards the west. This boulder weighed probably above 400 tons. Along the range of hills and up to their summits, at a height of about 600 feet above the sea on the south side of Loch Killesport, numerous boulders were seen from the road; but I was prevented going to them.

About 3 miles to the east of Ormsary, the road passes through the narrow valley of Auchloss, which runs in a direction about east and west. Smoothed rocks occur in this valley, on one of which Mr Alexander pointed out striæ running in a direction W. by N.

* In the "American Journal of Science and Arts" for 1877, reference is made to a boulder in Vermont, called "The Green Mountain Giant," weighing about 3400 tons; and to twelve still larger in New Hampshire—the largest measuring 62 × 50 × 40 feet, and estimated to weigh nearly 6000 tons.

These striæ seemed deeper at their west ends, as if the tools which cut them, had struck the rock first at these ends.

The boulders on Loch Killesport appeared to me, from their positions, to have all come from the westward. If they came on floating ice, the sea must at that time have stood at a high level to have floated ice of sufficient size to carry and deposit boulders of such weight as those above described. On that point there need be no difficulty, as there is abundant evidence that the sea prevailed over the Highlands of Scotland to a height of at least 2000 feet, and thereafter subsided, whether gradually or rapidly is not yet known. The sea-bottom on which the boulders were dropped, would of course present a very different surface from what forms the present dry land. What are now valleys in the land would be formed (after the sea subsided) by the detritus which filled these hollows being scoured out by rivers; whilst the boulders which had occupied the old sea-bed, when too heavy to be moved by river floods, would remain in the newly formed valley, though sometimes at lower levels. In like manner, the boulders which are now on the shores of sea lochs, may in many cases have been undermined by the scouring out of detritus by tides and storms, and sunk to a lower level than they originally occupied. Hence it is that along the *present* line of high water the boulders are generally more numerous than elsewhere; and the same circumstance occurs everywhere along the *old* sea-margin, as in Loch Killesport.

10. Another place visited was *Loch Swin*, an arm of the sea on the west coast of Argyleshire, about 16 miles west from Lochgilphead. Mr Alexander of Lochgilphead kindly accompanied me to this district also. At *Keills*, on the north bank of the loch, close to its mouth, there are several boulders of a light-coloured grey gneiss, and one or two of a fine-grained granite. The rocks on which they rest are a coarse dark-coloured Silurian.

The first boulder examined was on the shore facing the island of Jura, here distant about 4 miles to the west. Its size was $12 \times 10 \times 9$ feet. It lay on a bank sloping down towards the sea at an angle of about 15° to the W.N.W. It rests on Silurian rock, at a height of about 50 feet above the sea, and about 100 yards from the beach.

About a mile to the eastward, and not far from the old ruinous church of Keills, there is another grey gneiss boulder, $18 \times 15 \times 12$

feet, resting on a terrace about 150 feet above the sea. Another boulder, of fine-grained granite, lies near it on the same terrace.

Another boulder is on the shore here, 16 × 10 × 9 feet, with the longer axis lying E. and W. I learnt from Angus, shepherd at Keills (and who also acts as post messenger), that on the *Island of Dana*, at the south side of Loch Swin, there are three boulders larger in size than any on the north side.

On my return to Lochgilphead, I walked to *Carig Bay*, in the parish of North Knapdale, and on the hill to the S.W. facing the island of Jura was shown a fine-grained gneiss boulder, 12 × 6 × 6 feet, similar in composition to those at Keills. It was resting on a rocky surface sloping down to N.W. Its position suggested transport from N.N.W. Its height above the sea was 270 feet.

At *Tayvallich*, on the north shore of Loch Swin, I fell in with boulders forming two groups of 3 and 4 in number, whose relative position indicated transport of the uppermost from the west.

At *Scotnish*, also on Loch Swin, found an old sea-terrace at 42 feet above the sea, with a boulder on it, 18 × 11 × 8 feet, besides many others of smaller size.

In a short lateral valley opening into Loch Swin at *Loch Mhurrich*, I had shown to me by Dr J. M'Leod of Tayvallich an immense boulder, 36 × 15 × 13 feet, weighing about 500 tons. It rests on a knoll of shingle, and is about 30 feet above the sea-level, and distant from it about $\frac{1}{5}$th of a mile. This knoll is in the centre of a marshy meadow, which is surrounded by hills of from 260 to 300 feet in height, whose sides show beds of sand and gravel. The mouth of this small loch opens on Loch Swin to the W.S.W. The boulder is many hundred yards distant from the adjoining hills, so that there is no doubt that it is an erratic. But from what quarter, and by what means has it come? One naturally supposes that it must have come in by the mouth of the valley, of course at a time when the sea was deep enough to float it and lodge it in this *cul de sac*. The sea-bottom on which it dropped may then have been higher than the existing meadow; and as the detritus was washed away, the boulder may have protected the bed on which it rested, so as to form the present knoll.

I may add that there are numerous small lateral valleys along the north side of Loch Swin, extending a few hundred yards, and run-

ning in a N.E. and S.W. direction. They open into Loch Swin, and are well filled by boulders. These are generally most abundant on the south sides, and on the slopes of hills facing towards the N.W.

Berwickshire.

1. In *Ayton* parish, on Whitfield farm, 2 or 3 miles N.E. of the village, several small boulders of grey granite occur, about 270 feet above the sea. Nearest rock of same kind is on Cockburn Law, about 10 miles to W.N.W. Near Ayton Castle a bed of sand was excavated to the depth of about 20 feet and removed. Bits of coal (including cannel) were found in the bed, about 200 feet above the sea. Nearest place where coal strata occur, is in Mid-Lothian, on north side of Lammermuir Hills, about 30 miles to N.W.

2. In *Coldingham* parish, on Crosslaw farm, well rounded masses of hematite were turned up by the plough at a height of about 500 feet above the sea. Nearest place where hematite has been found is in East Lothian, about 30 miles to N.W. But the range of Lammermuir Hills intervenes. On this same farm, a boulder of white coal sandstone occurs, which must have come from East Lothian. Lumps of coal have also been found there in the boulder clay on Blackhill farm.

"On the heights east of Coldingham Loch, the rocks lie in separate and parallel ridges. The ridges are much rubbed and planed, especially on the N.W. exposures, as if some mighty force had battered and grated them down. There were also indications of striæ, which bore by compass nearly north, or N.½-W.—in this agreeing exactly with the striæ at St Abb's Head and the Farne Islands."—(*Address by Jas. Scott Robson, President of Berwickshire Nat. Club*, vol. vii. p. 175.)

3. In *Chirnside* parish, at Old Castles, there are numerous boulders of grey granite, from 1 to 2 tons in weight, and about 300 feet above the sea. Nearest rock of same kind is at Stanchal and Cockburn Law hills, visible from Old Castles, and about 8 miles distant to N.W.

4. At *Blackadder*, in Edrom parish, a boulder of blue whin or greenstone is on a knoll of gravel, on the west side of knoll. Its

height above ground is about 4 feet, and its width 2 feet. Level above the sea about 250 feet. Nearest rocks of same kind are at Hardens, about 5 miles to N.W., and about 500 feet above sea.

5. In *Hutton* parish, at Paxton brick-work, buried in boulder clay, a blue whinstone boulder, $7\frac{1}{2} \times 4\frac{1}{2} \times 3$ feet, weighing about 12 tons, was found. Its longer axis pointed N.W. by N. In that direction, Borthwick Hill near Dunse is situated, distant about 10 miles. It is the nearest spot for whinstone rock *in situ.* In the same brick-work, small boulders were found of old conglomerate, greywacke, chert, and white sandstone. Rocks of these kinds occur in Berwickshire to the westward. The brick-coloured porphyries of Kyles and Dirrington hills, situated about 15 miles to the west, were found there also.

Blocks of the same blue whinstone occur on the farms of Broadmeadows and Sunwick. Blocks of a peculiar greywacke, of a concretionary character and black in colour, occur in the Pistol plantations. The only rock of that kind known to exist is in the channel of the Whitadder, near Cockburn Law, about 14 miles distant to the N.W. Blocks of the same peculiar rock occur in great numbers on all the farms in the same line. There are specimens of them at Paxton House.

6. At *Stitchell Craggs*, pebbles of Old Red Sandstone are lying on the whinstone rocks, at a height of about 600 feet above the sea. Nearest place where Red Sandstone strata occur is some miles to the west. On the west sides of these craggs there are smoothed surfaces of whinstone rock dipping towards W.N.W. None are seen on any other side. At *Baillie Knowe*, in same parish, about 300 feet above the sea, a whinstone hill occurs, presenting on its west side similar smoothed portions of rock dipping W.N.W.

Cowdenknowes Hill, situated in Earlston parish, consisis of felspar porphyry. Large blocks of this rock are strewed over the muirs situated to the east, resting on Old Red Sandstone strata.

On *Smailholm Craggs*, about 3 miles west of Stitchell, at a height of 570 feet above the sea, rocks facing W.N.W. show striæ made by some agent coming from W.S.W.

On *Hume Castle Craggs*, at height of about 740 feet above the sea, there are rocks smoothed and striated, in an E. and W. direction.

"Boulders, carried a hundred miles and more from their native localities, are still found in many parts of Berwickshire, though by far the greater number, especially of the smaller ones, have been broken up for road metal. This is particularly the case along the post road between Reston and Ayton, where fragments of gneiss, mica slate, pure vein quartz, porphyries, and other rocks of Grampian origin, were, a few years ago, to be seen in every depôt by the road-side. The current which brought the ice upon which these were conveyed, must have come from the westward, where these rocks occur *in situ*. Among the more remarkable of these boulders may be mentioned a rounded block of gneiss on the road at the top of Ecclaw Edge,—a large block of mica slate on the slope of the hill east from Burnhouses,—several smaller masses at Windshiel, Kid-shielhaugh, and Abbey St Bathans,—and a block of a very peculiar diorite, formerly one of the stepping-stones in the River Whitadder at Ellenford. This diorite, which is composed of greyish quartz, red felspar, and a little chlorite, occurs *in situ* in the neighbourhood of Aberfoyle. Rounded pebbles of the same have been found in the Whitadder below Preston Bridge, where also mica slate, quartz, sandstone from the Lothians, &c., are to be met with in the river shingle."—(*Wm. Stevenson on Ice Action in Berwickshire*, "Berwick. Nat. Club Trans." vii. p. 209.)

Buteshire.

Arran.—Some years ago I spent a few days at Brodick and Corrie, and made the following observations:—

1. In travelling along the high road between Lamlash and Brodick, I observed thick beds of boulder-clay containing numerous boulders, the most prevalent being granite, and also a conglomerate, with large quartz pebbles in it. The height of these clay-beds was about 387 feet above the sea. Rocks *in situ* of the same nature are situated to the N.N.W.

To the south of Brodick Bay, there is a large number of Boulders, along and near the coast; but in Brodick Bay itself, there is a total absence of boulders, whilst to the north of Brodick Bay they are numerous.

This circumstance suggests a theory which will be mentioned, after

some account has been given of individual boulders remarkable for size or position.

One of the boulders to the south of Brodick Bay is known by the name of the Corriegill Boulder. It lies near the shore. Its highest point is about 15 feet above its base, and its girth is about 60 feet. Its shape is indicated by fig. 1, plate XIX., representing a section through it horizontally a little above the base. Its longer axis lies N.W. and S.E., with its sharpest end to N.W.

The boulder is granite of a grey colour, the ingredients being crystals of quartz, felspar, and mica, which are all rather larger than usual in size, and give to specimens a very coarse and rough aspect. It has a vein of finer grained granite running through it.

The top of the hill called Goatfell bears from this boulder N.N.W., and is distant about 4 miles. Granite occurs *in situ* on Goatfell.

Another boulder was measured, situated about half a mile to the north of the above on the shore at half-tide. It was $12 \times 9 \times 8$ feet. Its longer axis lay due north and south.

From this part of the coast, where these boulders begin to be numerous, the northern horn of Brodick Bay, at the sea-shore, bears N. by E. This horn is a continuation of a steep ridge which runs up to Goatfell.

2. To the south of Corrie (about a mile) two boulders of considerable size are situated on a plateau or terrace, which is from 89 to 96 feet above the sea.

The largest is shown on fig. 2, plate XIX., A being a horizontal section near the base, to show dimensions of the sides and their position by compass; B indicates the position of the greatest mass which is at the south end, the highest point there being 15 feet above the base.

The longest axis is in a direction about N. by W. and S. by E.

I calculated the weight of this boulder to be about 620 tons.

I omitted to mark the nature of the rock composing these two boulders; but they are, according to my recollection, grey granite.

Goatfell from their position bears about W. by S., and is distant about 3 miles.

The rocks of the district where these boulders lie are sandstone,

apparently carboniferous. There is a quarry for building purposes not far off.

These two boulders must have been *carried*, for there are no adjoining hills from which they could have fallen. Carried by a glacier they could not be, as they are not in a valley nor near any from which a glacier could have issued.

3. To the north of Corrie, about 2 miles, the road passes a large boulder called the Catstane, which is about 18 feet in height and 56 feet in girth. The late Dr Bryce estimated its weight at above 200 tons. I calculated its cubical contents to be 131 yards, and therefore its weight about 262 tons. It is very angular in shape; but I could not ascertain correctly the length and direction of its different sides.

On the beach near the last-named boulder, there is a granite boulder which I was able to measure with exactness. It is in height about 12 feet. Its longer axis lay in a N. and S. direction, its narrowest end being to the north. Its shape and the length of its sides are shown in fig. 3, plate XIX. I estimated its cubical contents at 106 yards, and its tonnage about 212 tons.

The boulder next larger in size at the same place is shown in fig. 4, plate XIX., A and B, where A represents the lengths of the different sides, and B gives an idea of the height, which was about 10 feet. The direction of the longer axis and of the narrowest end was much the same as in the other boulder.

Another granite boulder on the shore (at the old sea-margin of 12 feet above the present sea-level) is shown on fig 5, plate XIX., where A gives a horizontal section to show its shape and direction of its longer axis, and B its peculiar position, resting as it does on a mass of Red Sandstone (coarse) conglomerate strata, rising up towards the north. The position of the boulder, blocked as it is at its south end by the sandstone, shows that it has come from the north. The girth of this boulder is about 33 feet, its length about 9 feet, its width 8 feet, and its height 8 feet.

Many blocks of the conglomerate sandstone on which this boulder rests are found along the shore to the south, none to the north. It will not fail to be observed that one feature characterises all the cases of boulders just mentioned. The narrowest end points towards the north, suggesting the idea that, after being deposited, they

had been subjected to some agency which put them into a position enabling them to withstand any farther dislocation.

That this agency came from the north, their position clearly indicates, an inference confirmed by the transference towards the south of the sandstone blocks above mentioned.

4. On ascending the hills to the west of Corrie, I found smoothed surfaces on the sandstone rocks and traces of striæ at a height of about 158 feet above the sea. The direction of the striæ was N.W. and S.E.

On these hills, up to a height of about 587 feet above the sea, the boulders are very numerous. All that I examined were of grey granite, except three, and these were of conglomerate.

Between these hills and the high granitic boss to the west, reaching to a height of about 1800 feet, there is a valley running N. and S.

The high hill on the other side of the valley to the westward is composed of grey granite. I climbed this hill up to a height of 1270 feet. The ground passed over was thickly strewed with grey granite blocks. I could here distinguish two sets of boulders—*one* set angular, which may have fallen from the mountain—*another* set well rounded, which seemed to be erratics, not only because of their shape, but because they were of a harder texture than the rock of the hill. The ingredient crystals were also larger in size. These rounded blocks I observed to be on the hill-side for at least 100 feet above the point reached by me.

One of the boulders arrested my attention on account of its size and position. It was 25 feet long, 9 feet wide, and 12 feet high. This boulder and many others were lying with their longer axis N. and S., and could not, as it seemed to me, have fallen from any rocks on the hill above.

5. I went across the island to Loch Ranza, the summit-level being about 660 feet above the sea.

I was unable to examine any individual boulders. But I noticed that there were many more on the east side of the summit-level than on the west side.

I saw on the hills facing the N.W. numerous "perched blocks," at heights of from 1600 to 2000 feet. But they were too far off to admit of examination.

Near Loch Ranza some remarkable terraces, with boulders, arrested my attention, at heights of from 80 to 100 feet above the present sea-level. Great scaurs of gravel and sand, which were in beds, sometimes flat, sometimes dipping at a high angle, were under these terraces.

6. In connection with the Arran boulders, reference may be made to the following:—

Ailsa Craig (Ayrshire) is a mass of trap,—much of it (as I understand from Professor Heddle) being columnar porphyry of a white colour. It reaches to a height of 1114 feet. Not having visited it myself, I may be allowed to refer to information given by others.

In a paper by Mr W. N. Macartney, in the "Proceedings of the Glasgow Nat. Hist. Society for 1868," it is stated that the Craig bears many marks of glaciation, up to near the top. On the north side there is, at the height of about 600 feet, a deposit of boulder-clay in a slight depression of the rock, and guarded by a boss of rock from any currents, which, when the Craig was submerged, may have flowed from the N.W. This deposit is of a red colour, and composed of sand and clay, derived probably from the Old Red Sandstone rocks situated in Arran and other islands to the north. In this deposit, Mr Macartney says he gathered a number of pebbles, striated or scratched, consisting of quartz, and metamorphosed slates and shales.

Mr Wünsch of Glasgow informed me that he had found granite pebbles on Arran.

(2.) At Ardrossan (Ayrshire), on the beach, there are numbers of conglomerate boulders, distinguishable by the prevalence of white quartz pebbles in the rock.

At Lamlash Bay, in Arran, I noticed boulders of a similar conglomerate.

Have they all come from some northern quarter?

(3.) At Millport (Buteshire) there are two large boulders of coarse grey granite, which are used in the harbour there as "*pauls*" for ropes from ships.

(4.) Near Beith (Ayrshire) there is a hill called *Cuffs*, which Mr Craig of Beith took me to visit. On the north side of this hill he pointed out many small-sized boulders of grey granite at a height of about 560 feet above the sea. The felspar crystals in it are

of a large size and very white colour, much resembling those found in the Arran boulders. Cuffs Hill consists of porphyry. It is surrounded by Carboniferous strata.

7. On a review of the facts stated in these notes regarding the Arran boulders, it seems probable that those described had been brought from the north, judging by the way in which they lie, and also by their composition.

With reference to the absence of boulders from Brodick Bay, and to their abounding along the coast both north and south of that bay, what occurred to me was, that if the boulders were brought from the north by floating ice, the rocky ridge running down from Goatfell peak (a mountain 2874 feet high) to the north point of Brodick might have had the effect of diverting the current in a S.E. direction, which would carry the ice beyond the bay. That bay is at the lower end of a valley which runs up among the highest hills; and if the theory of glacier from these hills be adopted, the bay should have been crowded with boulders, instead of being free from them.

Big Cumbrae.—I was guided to the north end of the island by the Rev. Mr Lytteil. There, on the farms of Figgatoch and Balloch Martin, I found several large boulders of mica schist lying on Old Red Sandstone rocks. The largest measured 12 × 6 × 3 feet. But it may have been larger, much of it being below the surface of the ground. The longer axis lay N.N.E., which was also the direction of the hollow or small valley in which it lay.

On the 70 feet terrace one of the schist boulders was about 5 feet square.

At the S.W. point of the island (viz., Kennery point), above half a mile to the west of Millport, I found several other schist boulders, on the old 12 feet sea-terrace.

Little Cumbrae.—The rocks of this island are entirely a brittle claystone trap. The rocks at the highest part (near an old tower), at a height of about 400 feet above the sea, are very distinctly smoothed and grooved. Most of the smoothed surfaces slope down towards and face N. by W.

The only part of the island on which striæ were found is at the east side, near a small ruined fortress. A hollow or trench occurs between the knoll on which that ruin stands and the main body of

the island. Fig. 6, plate XIX., shows the trench apparently scooped out in the rock by some heavy agent which has passed through, smoothing it on both sides and striating it on one side. The direction of the trench is N.E. by N. As it is only the east side which shows striation, the striating agent, if it came from the north, must have moved from a north-westerly point.

The striæ can be traced longitudinally for about 30 yards.

The figure shows a boulder, B, resting on an upper part of the trench, where there happens to be a sort of shelf where it has originally been lodged.

This is the "Split Boulder" first noticed and described by Mr Smith of Jordanhill. Before it broke into its two fragments, its size must have been 8 × 7 × 6 feet. Though the boulder is a claystone trap, viz., the same rock as that composing the main body of the island, I do not think it has rolled down to its present position, but agree with Mr Smith, that it is a true erratic, having been brought by ice which probably jammed in the trench as it was passing through.

The island has a number of Old Red Sandstone and also of conglomerate boulders on various parts of it, very similar in mineralogical character to the strata which are seen on the shore to the N.N.W. at Rothesay and Toward. One of these conglomerate boulders is of archæological interest. It bears the name of the Belstane, and is supposed to have been in former times connected with the Beltane fires. There are markings on the stone which have evidently been made for some special purpose. One of these boulders, about 5 feet square, rests on rock, and may have been used as a "Rocking Stone." The Rev. Mr Lytteil pointed out this stone to me.

East Lothian and Mid-Lothian.

1. For a notice of several boulders see paper by Convener in "Proceedings of Edinburgh Royal Society" (7th July 1877).

2. Extract from paper on the "Physiognomy of the Lothians," by R. J. Hay Cunningham, in "Trans. of Wernerian Society for 1838," vol. vii.

"In this district little extent of country can be passed without

numerous rolled masses of rock occurring, which are not found *in situ*, but only in distant localities.

"On the coasts of Linlithgow and Mid-Lothian, in the valleys of the Pentlands and on their acclivities, and on the flanks of the Lammermuirs and Moorfoot range, we easily detect rolled fragments of granite, syenite, porphyry, mica slate, gneiss, quartz-rock, and varieties of greywacke, which are met with only in the central districts of Scotland, while an examination of them shows that they decrease both in magnitude and frequency, as we advance southward; a fact indicating that the aqueous currents (for to such only can they be referred) diminished in intensity as they were removed from the central parts of the island."

Professor Nicol of Aberdeen (in the "London Geological Society's Journal" for 1848, vol. v. p. 23) refers to "one angular block of mica slate, near Habbie's How, on the Pentlands, weighing (according to a measurement I made) 6 or 8 tons. Farther west, I found another block, also angular, of the same sort, weighing about $\frac{3}{4}$ of a ton. When it is considered that these masses must have been carried upwards of 40 miles, floating ice seems to be the only agent to which their transport can be ascribed. Blocks of a smaller size are very common;—some are of kinds of rock *which I have never seen in Scotland.* On one hill, 1500 to 1600 feet high, I found these travelled stones particularly abundant, *and apparently increasing in number from below upwards.* In some places they appeared to form broad bands running nearly in straight lines from N.N.W. to S.S.E., and without any reference to the present declivity of the ground, except *becoming more numerous towards the summit of the ridge.* These blocks consisted chiefly of trap rocks, especially basalt; the hill on which they rested being a red felspar or claystone porphyry."

3. On 29th Oct. 1879, the island of Inchkeith was visited, under the guidance of Colonel Moggridge, R.E., superintending the erection of fortifications there. The rocks consist chiefly of basalt and porphyry intruded among the Coal-measures of Fife and Mid-Lothian. In various places the rocks are covered with beds of boulder-clay, gravel, and occasionally sand. The inspector of works (Mr Beck) mentioned that at the east end of the island, when removing a bed of shingle (about 60 feet above the sea), he

picked up two pebbles of red granite about the size of a hen's egg. Thinking it curious that granite should be found there, he laid the pebbles aside and kept them for some time, but they had since been mislaid.

Having been told that a number of large pebbles of various kinds were seen at the west end of the island, on the beach, I went there, and found numerous pebbles of granite (both red and grey), gneiss, quartz, and hard Silurian rocks.

On the highest part of the island (which is 182 feet above the sea), and on portions facing the N.W., the rocks have been well planed down to even surfaces by some agency from the west. But no striæ were observed.

4. A short time ago I went, on the invitation of Captain John Macnair of Edinburgh, to examine two boulders lying at the side of the Water of Leith, on the farm of Whelpside, near Kaims and Dalmahoy hills, about 9 miles S.W. of Edinburgh. One boulder was $13 \times 10 \times 6$ feet, and the other $10 \times 8 \times 5$ feet; but the depth of either could not be well ascertained, being deeply sunk. They were both of them a hard porphyry, containing minute crystals of a black mineral like hornblende, in a basin of white felspar. The longer axis of both was E. and W. They were covered with striæ, long and deep, running also E. and W., and indicating a movement over them from due west.

I ascended the Kaims hill, situated about a mile to the N.W. of the boulders, and found its west side swept bare, with numerous large fragments of the rock of the hill (a hard sandstone) strewed over its eastern slope.

On my way back to Edinburgh I examined the whinstone quarry of Ravelrig, and found, on the natural surface of the rock composing the hill there, numerous examples of ruts and scoopings, all indicating an agency which had passed over the hill from due west.

Kirkcudbrightshire.

Large rounded fragments of granites and syenites are abundantly scattered over the Stewartry, and so arranged as to indicate that they have been dispersed by a force proceeding from the N.W.—

(Robert J. Hay Cunningham, "Highland and Agricultural Society's Trans." vol. viii. p. 716.)

Peeblesshire.

Reference made "to the boulders of gneiss, granite, and mica slate, which belong to rocks unknown in the hills of that county, and several tons in weight." They "seem to require for their transport more powerful agents than mere currents of running water. We can scarcely conceive these possessed of sufficent velocity to convey masses of such a shape and size along a level plain, still less over the summit of hills 1500 or 1600 feet above the level of the sea, and across many winding valleys. The most probable means of conveyance, not only for these, but for many of the smaller fragments, seems to be masses of ice floating in an ancient sea, by which the highest summits of these hills were then submerged."—(Professor Nicol, "Highland and Agricultural Society's Trans." vol. viii. p. 197.)

Roxburghshire.

1. Near Castleton, many blocks of granite—both red and grey—lie on the greywacke and also the carboniferous rocks, which must have come from hills to the westward in Dumfriesshire or Kirkcudbrightshire, 30 to 60 miles distant, crossing the valley of the Esk.

2. On Ruberslaw, a hill of greenstone, about 200 feet below the top, I fell in some years ago with a large block of greywacke. It was lying on Old Red Sandstone strata. The nearest greywacke rock is situated to the westward about 3 miles. Between these rocks and the position of the boulder, there is low ground, at least 800 feet below the level of the boulder, which it must have crossed to reach its site.—("Edin. Roy. Soc. Trans." vol. xv. p. 454.)

3. Near the village of Nesbit, about 8 miles S.W. of Kelso, there is a boulder of small-grained greenstone 8 × 7 × 5 feet, identical in composition with the rock of Penielheugh, a hill on which stands the Waterloo pillar, a structure of about 120 feet in height. The rocks where the boulder lies consist of Old Red Sandstone;

and they are well covered by beds of gravel and sand. The boulder is on a knoll, near the top, but a little to the N.W. of it. The longer axis is in a direction S.W. and N.E. Penielheugh Hill is situated to the S.W. and distant about a mile from the boulder. The hill is 774 feet above the sea—the boulder 224 feet above the sea. The exposed rock of the hill on its west side reaches down to about 400 feet above the sea.

That the boulder has been brought to its present site from Penielheugh, is evident,—the composition of the rock being the same in both. The Old Red Sandstone rocks which prevail generally in the district, reach up to within about 100 feet of the top of Penielheugh, but only on the *east* side. These strata are entirely absent on the *west* side, suggesting, therefore, the probability that the west side of the hill has been denuded of them by some agency which has come against the hill from the westward. This inference is confirmed by the fact, that on the sides of the hill facing the west, the igneous rocks are all *bared*, and many of them *smoothed;* whilst on the sides facing the east, no igneous rocks are visible, being covered by sandstone strata, with drift materials over these.

These facts will be better understood by reference to plate XVII. fig. 6, where P represents Penielheugh Hill, B the boulder. The strata in dark colour is the Old Red Sandstone formation.

On looking from the top of Penielheugh westward, a wide valley is seen in that direction, the Eildon Hills on the north, and the Minto Hills on the south.

Through that valley some agency has undoubtedly come, impinging with great force on Penielheugh; but whether a local glacier or a sea-current with floating ice, there is nothing to show, though the extensive beds of gravel and sand which abound in this district, at no great distance from Nesbit, seem rather to favour the latter theory.

Selkirkshire.

On the top of Meigle Hill, about 2 miles from Galashiels, there is a boulder which I was requested to come and examine. It is of this shape, and its size is $6 \times 4\frac{1}{2} \times 3\frac{3}{4}$ feet.

Its longer axis lies N.W. and S.E., the sharp end pointing N.W. The person who invited me to visit the boulder, and guided me to it, told me that he had, by means of a lever, moved the boulder about 9 inches from its original natural position. The boulder is a hard grey Silurian rock, much harder than the rock of the hill, which is also Silurian.

The boulder, being well rounded, seems to have undergone much friction; and there are hollows and scoopings on several parts, such as frequently occur on rocks long subject to the eddying action of water. The boulder is about 58 yards east from the apex of the hill. It appeared to be lying on gravel or other drift materials, and about 12 feet below the apex of the hill. The hill reaches to a height of about 1430 feet above the sea. Many other boulders occur near the top of the hill, all of the same Silurian rock, well rounded, but none quite so large as the one above described. Meigle Hill stands by itself, *i.e.*, there are no other hills of equal altitude within some miles. There can be no doubt that all the above mentioned are "*erratics*," but from what quarter brought there is nothing to show. It would be difficult, however, to conceive any other medium of transport than floating ice.

Perth and Stirling Shires.

In looking through the Committee's previous Reports, I find reference made to a boulder near Doune, a *conglomerate*, weighing about 900 tons. A full account of this boulder, of the gravel beds on which it lies, and of its probable parent rock, is given in my little book called "Estuary of the Forth" (Edmonstone & Douglas, 1871), to which it may be allowable to refer (page 41). There are,

besides many other *conglomerate* boulders—as at the following places:—

On Landrick Estate, one weighing about 360 tons (p. 43).

At Keltie Bridge (a mile east of Callander), one weighing about 60 tons (p. 45).

On Gartincaber estate, one weighing about 16 tons (p. 43).

On north side of Teith, below Landrick Castle, one weighing about 13 tons (p. 44).

In the Burn of Cambus, two weighing about 13 and 24 tons respectively (p. 44).

In the district traversed by the hill road between Doune and Callander, there are multitudes of *conglomerate boulders* of smaller size (p. 44).

At Cornton brick-work (between Stirling and the Bridge of Allan) I saw a small *conglomerate boulder* found in the clay-bed there.

On the rocks adjoining Stirling Castle on the north, I observed small *conglomerate boulders*, besides some of gneiss and greywacke (p. 39). At Loch Coulter and Gillies Hill, places about 3 miles south from Stirling, and from 400 to 600 feet above the sea, I found several *conglomerate boulders*, besides some of mica slate and felspar porphyry, evidently all brought from the N.W.

On Plean estate (4 miles S.E. of Stirling), besides boulders of granite, gneiss, greywacke, and whinstone, there were some of *conglomerate* (p. 46).

At Glenbernie, near Torwood (5 miles S.S.E. of Stirling), I found a *conglomerate boulder* about 6 feet square (p. 48).

On Dunmore estate (about 9 miles S.E. of Stirling) there is the Carlin Stone, a *conglomerate boulder* weighing about 10 tons.

This list of *conglomerate* boulders may be considered interesting, as the position of the parent rock is known, viz., the band which traverses the country at Callendar, running from that point N.E. towards Brackland, and S.W. towards Aberfoyle and Loch Lomond.

Assuming that the boulders have all come from this band of conglomerate rock, they show a transport from the N.W. They also show that they cover a wide district of country towards the S.E., not a district forming a valley, in which a glacier might have moved,

but a district at various heights above the sea from 20 to 600 feet or more.

The boulders seem to increase in size and number the nearer they are to the parent rocks.

In the accounts given of these boulders it will be seen that those which are somewhat elongated in shape, have their longer axis lying N.W. and S.E., and that where striæ occur, either on boulder-clay or on rocks, these striæ lie in the same direction (pp. 46, 60, 61, 64).

The conglomerate boulders are chiefly referred to, because they are the most numerous, and the position of their parent rocks is best known. But the other boulders of the district—granites, silurians, and porphyries,—all yield confirmatory testimony, as will be seen from the positions of their parent rocks, and also their own position.

One other feature in this district may be mentioned, viz., the direction in which the gravel-beds have been by some means scoured out, leaving escars or kaims. Thus (1) at and near Bucklyvie (10 miles west of Stirling) there are three elongated knolls of gravel, sand, and boulders, lying in an E. and W. direction, reaching a height of from 60 to 70 feet above the adjoining district.

(2). On Blair-Drummond lands, there is a knoll composed chiefly of sandstone rock, but partially covered with gravel. It is known by the classical name of the Naidds' Knoll, given probably by Lord Kames, a former proprietor. It is in length 90 yards, and in extreme height about 50 feet, with a width of about 40 yards at its greatest width, which is near the east end. The direction of the longer axis of this knoll is W.N.W. and E.S.E. At the head of the valley towards the west, the lowest level is what is called the Pass of Bolat, and that point bears W.N.W. from the knoll. The rock of the knoll is a soft red sandstone, which could have been worn into its present shape by a current flowing through the pass in an easterly direction.

(3). About 2 miles south of Stirling, there is a gravel hill called Coxit. Its length is about $\frac{3}{4}$ of a mile, and its greatest width 300 yards. Its height is from 80 to 100 feet. Its longer axis runs about N.W. and S. E. A current flowing from the westward down the valley upon Stirling Castle rocks, might have had a branch diverted towards the S.E., and have scoured out the drift deposits, as it flowed near St Ninians and Sauchie, leaving Coxit Hill as a

remnant of the drift. Between Sauchie and Gillies Hills (chiefly whinstone), which are near Coxit, there is a narrow valley running in a direction N.W. and S.E., which would help to guide a current running in the direction supposed.

(4). The long escar of gravel passing through Callendar Park and Polmont, extending for about 2 miles, runs in an east and west direction, because there, any current would flow in a direction approximatively parallel with the axis of the valley of the Forth.

II. *PROFESSOR HEDDLE'S NOTES.*

Ayrshire.

1. In the *Valley of the Stinchar*, a boulder of fine-grained claystone, about a cubic yard in size, lies near the hamlet of Poundland.

It seemed to be in its mineralogical character identical with the rock of the hill of Glassal, situated to the N.E., and also with a rock on the shore to the west near Bennane Head.

2. About half a mile to N. of Colmonell, at a height above the sea of about 200 feet, a dolerite boulder occurs 27 × 23 × 12 feet. Its longer axis lies N. and S.

It lies on till, and the till covers the serpentine rock of the S. slopes of Belhannie Hill.

A small boulder, apparently a fragment of the larger, lies to the south.

About 600 yards E. by S. of this boulder, viz., up the valley, a spur of the same kind of rock projects out of the serpentine of the hill.

3. Lower down the valley there is another boulder of the same rock. It has been rent into four pieces, and the impression is suggested that it had been rent in consequence of falling from a height. It also rests on till. The fragments indicate the boulder before being broken to have been 21 × 21 × 10 feet in size. Its long axis is also N. and S.

4. On the shore, a little to the north of Lendalfoot, there lies an Old Red Sandstone conglomerate boulder, 8 × 6 × 6 feet. It is undis-

tinguishable from the conglomerate of Wemyss Bay, situated about 30 miles to the north.

Argyleshire.

Colonsay.—The rocks near the place in this island where the steamboat calls, viz., on the N.E. side, were found to have been smoothed in a line bearing W.N.W. and E.S.E.; but from which direction the smoothing agents had come was not ascertained.

North Uist.—At Loch Maddy the rocks were found to have been smoothed every where, and in the same line as at Colonsay.

There are localities which show unmistakably that the smoothing agent had followed a course from west to east, or rather from the north of west. But the hollows or trenches between the higher grounds and the strike of the old gneiss strata have exercised some influence in diverting the smoothing agent, sometimes one way and sometimes another.

The two trap islets, Maddy More and Maddy Beg, porpoise-nosed to the west, and cliffy to the east, vouch for the direction of flow of the agent which conferred upon them their striking forms.

Whilst at Loch Maddy, I was accompanied by Mr Harvey Brown, who has written several well-known works on the natural history of Scotland, and has noted glaciation with an active eye, and an intelligent and independent mind.

Mr Brown had lately returned from a visit of some duration to Newton, on the coast of North Uist, where he had Mr James Thomson of Glasgow as a companion. He furnished me with a sketch and description of a boulder which lies on sloping ground to the S.E. of Newton. It is $13 \times 5 \times 4$ feet. It lies with its longer axis pointing N.N.W. and S.S.E. Another is $9 \times 5 \times 5$ feet. He stated that Mr Thomson and he had spent some time in examining the glaciation of the neighbouring shore, and found that all the rocks were glaciated from the N.W. He suggested my applying to Mr Thomson for farther information. Mr Thomson, in reply, stated that the glaciation on the west shore of the Long Island was all from the west, varying occasionally between N.W. and S.W., and he added an expression of surprise that any one could have made the mistake of not seeing this fact, it was so palpably evident.

Harris.—On Gilebhal Glass, the southern flanks are striated up

to a height of 500 feet, apparently by ice which came through the gorge of Tarbert from the west. Above this height the glacial striæ strike down the slopes of the hill in every direction.

On the S.E. slopes of the hill, there are portions of the ribbed and striated rock which have been torn up, and carried but a short distance, then let down and fractured in the fall.

Clisham and Langa have but few boulders; those on the south spur of Langa reach a height of 1400 feet, which is nearly the upper limit of the glaciation of these hills.

While the glaciation of the east and west trenches between the Harris hills shows a course of transit from west to east, the valleys of the *highest* hills showed ice to have passed down them from the higher level, whatever the direction of these valleys may be.

West Loch Tarbert.—Though rock does appear between the eastern and western arm of the sea which impinge here so closely upon one another as to warrant the above common appellation, yet the isthmus is for the most part made up of boulder-studded till. One or two of the boulders are of a close-grained hornblendic rock, and doubtless have been portions of a band of rock of an identical character situated a few hundred yards westward on the north shore of Loch Tarbert.

As the nature and structure of this crypto-crystalline bed is very marked and unmistakable, I regard the above as unimpeachable evidence of the course of the ice through the pass; and it must stand as such till a similar bed is found on the shores of East Loch Tarbert. For such I searched without success, though I found a characteristic bed of graphic granite, no fragment of which, however, did I find in the till which plugs the throat of the pass.

Glen Scramble.—This deep glen lies between Gilabhal Glass and Skiam Hill. At the bridge which crosses the stream issuing from the glen, I found a number of loose masses of an igneous rock, identical with a rock forming a dyke coming out above the bridge. These masses, therefore, have come down the glen, viz., from the east; but if they were brought down by ice, there could have been no great mass of ice, the distance of conveyance being quite trifling in amount.

Scalpa Island.—Walking eastward from the village, I fell in with a boulder, 7 × 6 × 6 feet, of a characteristic granite, butted up against the rocky steps of a small knoll of gneiss rocks on its east side, about 35 feet above H.W. mark. A sketch of this boulder is given

on plate XVIII. fig. 6. On the face of the hill directly opposite on the Harris shore, situated to the N.W., a great bed of the same granite rock is distinctly visible. Two other granite boulders similarly "stopped" occur to the east of this one.

Shiant Islands.—On the *upper* surface of these islands, three in number, all of basaltic trap, and reaching to a height of about 500 feet, there are no boulders of any *foreign* rock. On the southern island there is a line of boulders not much rounded, which lie directly east of a spot where there has been a palpable rending.

Two of the islands are connected by a ridge or "ayre" of loose materials, over which the waves now occasionally roll. On the western slope of this "ayre" there are much worn fragments of two foreign rocks, viz., hornblendic gneiss and Cambrian sandstone.

The gneiss blocks are about 2 cubic feet in size. The conglomerate blocks are sometimes as small as eggs; two of these, but none of the gneiss, were found on the east side of the "ayre."

On a stretch of shore along the N.W. side of the most northern island, conglomerate blocks also occur.

The only place in this part of Scotland where I know of a similar conglomerate rock is on the Eye Peninsula of Lewis, a short distance east of Stornoway, and about 30 miles to the north of the Shiants.

There is one other feature about the Shiant Islands which seems worthy of notice. The two highest islands, viz., *Garbh Eilan* (rough island), and *Eilan an Tighe*, lie north and south of one another; whilst the third, viz., *Eilan Mhuire*, lies to the east, and does not reach so high a level as the other two.

The upper surface of the two largest and highest islands, both when viewed from a distance and when examined in detail, present such soft and gently-sweeping risings and hollows that ice in some form or other appeared to have passed over and pressed on the surface of the rocks. It had evidently gone over *Eilan an Tighe* from W. to E., and over the southern part of *Garbh Eilan* (lying to the north) in the same direction, but over the higher parts and main bulk of the island from the S.W.

This movement and direction of the ice on these islands is corroborated by the position of a number of boulders on both of these islands consisting of the basaltic rock of the islands, which are all on the east side of the most southerly of these islands, and in the

more northerly to the east of one or more spots where there has been a palpable rending of rocks by some powerful agent moving on them from the westward.

Now, it is rather remarkable that the island of *Eilan Mhuire*, situated to the east of the other two islands, presents on its surface no traces of the same smoothing which occur on the other two islands. It is lower in level than the other two. If it was ice which passed over and rubbed on them moving towards the east, why did not it also pass over and rub on *Eilan Mhuire?* If it was a sheet of land ice, the fact of *Eilan Mhuire* being a little lower in level should rather have ensured contact by the ice. If, however, the ice was floating, it may have passed over the lower island without reaching it.

Skye.—An examination of the north-east part of the island from Aird Point to Portree was made, chiefly along the coast, and partially among the hills.

While there was found throughout evidence of vast denudation with frequent rounded contours (as along the line of cliffs above the Kilt rock), the rocks nowhere bore groovings or even scratchings.

The cols between the numerous heights of the central range of hills were narrowly examined, as, in the case of a movement across the island from either N.W. or N.E. these hills must have been subjected to a great amount of "scour."

At the several cols, averaging about 1300 feet above the sea, the water-sheds which fall to the south commence with the most singular precipitancy, there being hardly a yard or two between the brink of the precipice (which falls sheer to the N.E.) and the trickling of a marshy stream flowing in the opposite direction. Between many of these cols, peaks of rock shot up to a height of 2000 feet and more.

There were no hollows and no contours which could be assigned to ice. The slope on both sides of the stream-trench was such as would result merely from the sliding soak of water.

No true boulders were any where to be seen. That the summit of this range has not been ice-worn, may be deduced from the abruptness with which fragments of an upper bed of basaltic columns shoot up with a pillared steepness which show no rounding of their angles, or abrasion of any of their terminations.

A loose pillar (of which a sketch was taken) points the same way.

This pillar, retaining all its original sharpness of angle, lies on its side at the very highest part of the whole range.

Though there is no evidence that ice has been over the top of these cliffs, there is evidence that it has been at the *bottom.*

The southern shore of Stainchol Bay is, with the little island at its eastern horn, stretched like a half-opened hand, so as to catch everything which may have been carried from the north along the eastern shore.

Among the rounded masses lying on the beach, there are blocks of the same Cambrian conglomerate which occurs at the Shiant Islands, and of a larger size.

On account of the position of Stainchol Island, it is not likely that these could have come from any point east of north.

On the island itself, no boulders were seen except on the S.W. shore; several consisting of dolerite, in which labradorite is well seen, lie here. A rock of the same nature occurs about 50 yards to N.N.W.

Loch Torridon and Loch Maree.—The position of "*The thousand hills*" (consisting of dirt cones and delta heaps) in Glen Torridon, and the smoothed rocks at the head of the glen, leave no room for doubt that a true glacier had descended this glen from the north and east. But, on the other hand, the till at the very summit-level between Glen Docharty and the head of Loch Roisk, has indubitably been water-dressed, and the dressing agent seems to have come up Glen Docharty.

The ice had apparently come out of every corry of the eastern sides of Leagach and Eye, to merge into the Torridon glacier.

But, on the other hand, there were found on *Scuir na Convaran* (a N.E. quartzite spur of *Ben Eye*) boulders of hornblende rock, hornblendic gneiss, and of Cambrian sandstone.

The hornblendic boulders were very similar to the hornblende of *Ben Arrichar* on the north shore of Maree, 13 miles to the westward.

As they lay much in line, in order to ascertain that they were not merely the turned-over fragments of a vein, though such a thing was most improbable, the ground was carefully scanned by several pairs of eyes, but no fixed mass was found.

An opposing spur of *Miall Ghubhais*, called *Carn a liadh* (grey

cairns), which lies N.W. of this, was distinctly hummocked at a height of about 950 feet above the sea.

Black Mount district,—having Loch Levin on the north, Moor of Rannoch on the east, and the Linnhe Loch on the west.

1. A train of boulders having been noticed by me on the north slopes of the valley of the *Beathard*, west of Loch Tulla, viz., on the low slopes of *Stob Ghabhar* and *Ben Toaig*, and also several huge blocks upon the shores of *Loch Dochard*, I felt a desire to seek for the parent rocks.

The boulders on *Stob Ghabhar* were of a pecular white granite, and were in size on an average up to 10 × 10 × 7 feet.

Ben Toaig and *Ben Terrick* are hills of gneiss. In the col between these hills, at a height of 2530 feet, the same variety of white granite boulders were found, with an average size of about a cubic yard, much worn. There was glaciation on the rocks (but much effaced), from S.W. to N.E.

Stob Ghabhar is also a gneiss hill. No boulders were seen except on its southern slopes, *i.e.*, at the spot already mentioned.

Ben Starrav was ascended. Its rocks were different from that of the boulders, as they consisted of a flesh-coloured granite.

The hills called *Scon Ghearraen* and *Meal Odhar* were next examined, forming west spurs from *Stob Ghabhar*. The rocks on them, as well as on *Glass Bein Mohr*, were found to be granite, but not exactly the same as that of the boulders.

Albannach hill was found, from its first eastern cliff to its summit, to consist of granite *identical with that of the boulders*. Blocks of the rock strewed its cross-corries in numbers; and the whole process of boulder formation may be said to be still displayed upon its slopes.

On the east and south-east sides of the hill there seemed to have been ice moving towards the south and towards the east.

On its northern side, similar traces were visible in the great corry under the sharp peak, showing a movement first to the north, and then a confluence with glaciation from a west corry of *Meall Targuinn*, thereafter curving westward, and sweeping towards Glen Etive.

This great hill, reaching to a height of 3425 feet above the sea, seems to have been the cradle of local glaciers, and also the source from

which the boulders near Tulla had been carried about ten miles in a direction E.S.E.

As it was thought desirable to see whether these boulders could be traced farther to the eastward, I tracked them back to the west and north shores of the lake, and thereafter for 2 or 3 miles up the course of the water of Tulla. They evidently diminished in numbers towards the east. Some of the boulders at Loch Tulla were about 8 cubic feet in size.

A search was next made along the southern range of hills, of which Meal Buідh, Ben Creachan, and Ben Achallater are the highest. But no boulders of the same or of any kind were found on them.

These boulders, therefore, had been carried, as it were, in a stream, and one of no great width, towards the S.E.

The valley, which gradually ascends westward from Loch Tulla towards the great massive hill of Starrav, becomes very narrow immediately to the east of Loch Dochard.

If any powerful agent passed through this valley eastward, it is probable that there would be great obstruction and a violent pressure on and rending of the adjoining rocks.

The lower part of the pass contains much till, and occasionally rock rises up through the till with finely smoothed hunches, showing striations from the W.N.W.

On the south side of the lake there are some enormous boulders, mostly angular, several of which are broken or fractured, as if by falling from a height. A sketch is given of one of these, fig. 7 on plate XVIII., as it is the largest I have seen or heard of in Scotland, except one in Arran. Its size is $45 \times 22 \times 26$ feet, and amounting therefore in weight to about 1900 tons. It consists of mica gneiss, and lies upon till. The view in the figure is taken from N.N.W. Other boulders of a similar rock occur at the same place, nearly equally large.

The hill immediately to the south of this boulder is composed of a similar sort of rock; so that very possibly, nay probably, the boulder may have been detached from the hill. But it is so far from the hill, and the intervening ground is of such a nature, that nothing but ice could have brought it into its present position.

The rocks at this place are much rounded, and show striæ running

W.N.W. and E.S.E. The striating agent unquestionably here came from the westward.

2. *Loch Creran.*—On the east side of this loch there are a number of boulders, some of very large size, of which notice was taken in the Committee's two last Reports.

My attention was drawn to these by our Convener, so that in the event of my visiting that district during the past summer I might endeavour to discover from what quarter these boulders had come.

I was glad to find myself able to comply with this request, and I spent several days in examining the district in question.

On the banks of the Creran there are two distinct classes of boulders, differing in mineralogical composition.

Those in the lower part of Glen Creran, near the bridge at the head of the loch and between Invercreran House and Fasnacloich, are much weather-worn, dense in structure, and dark in colour. The hornblende in them is dark-brown in colour, with but little felspar, and they contain a little bronzy biotite.

In a higher part of the glen, at and above Fasnacloich House, the boulders have much felspar, which is pale in colour; also hornblende which is always green, sometimes light-green, and a little quartz, but almost no biotite.

The rocks of the glen adjoining the places where both sets of boulders lie are quite different from the rocks composing the boulders; I therefore made a diligent search among the hills in the neighbourhood for the parent rocks.

The first-mentioned set of boulders, which I may call the *Invercreran* boulders, I found as regards mineral composition to be the same, or very nearly the same, as a band of rock in the *Coire Dhu* of *Fraochaid*, at a height of from 1500 to 1700 feet above the sea. This corry leads up from Glen Creran about 4 or 5 miles to the N.N.E. of Invercreran. The only mineralogical difference which I could detect was, that in the rocks on the hill, there was perhaps rather less biotite.

The place where the rock composing the Fasnacloich boulders was found is in a col lying a little north of the fountain-tarn of the River Durer, a river running into the Linnhe Loch at Coil Bay. The col lies between *Stob Coire Dhu* and *Stob Coire Ruadh*, at a height

of 1940 feet. A number of blocks of this rock were found by me at the west foot of *Miall an Aodain*, a hill situated to the eastward.

How these two sets of boulders have been carried to their present positions is a question on which I have yet formed no decided opinion. As perhaps bearing on that question, however, it is right to mention that the rocks near the fountain-tarn of the River Durer, at a height of 1940 feet, are much glaciated and apparently from the west.

Striæ occur on a clay-slate rock about $\frac{1}{4}$ of a mile south from the place just mentioned, just before the ascent of *Stob Coire Ruadh* commences, and at a height of nearly 2000 feet, which show a movement from a little to the north of west. These facts seem to suggest that some powerful smoothing and striating agent had passed over this district from the west, and at a level exceeding 2000 feet above the sea. But west from the place where these smoothed and striated rocks occur, there are no hills so high as to produce a glacier, unless, indeed, a glacier had come through Glen Tarbert, which is a continuation of Loch Sunart, and crossed what is now the Linnhe Loch. Loch Sunart and Glen Tarbert occupy a hollow in the district which runs in a direction about W.N.W. and E.S.E.

It is, however, proper to add, that on the rock where these W.N.W. striæ occur, there are cross striæ overlying and cutting into these, which cross striæ indicate a movement from the S.W. These cross striæ being more sharp and minute than those first made, indicate more recent and also less powerful action. Can it have been that a sea existed at a level exceeding 2000 feet above the present level, with ice in it which was floating about in eddying currents, among what are now high peaked hills, tearing rocks out of the shallows, and pushing them over what were then submarine reefs?

In regard to the boulders at Ivercreran and Fasnacloich, they manifestly have come from the particular hills above specified; but whether dropped from floating ice, or carried by glaciers, it is with our present information impossible to say.

The striæ last mentioned, as occurring at the height of 2000 feet, pointing about W.N.W., bear on the top of *Fraochaidh*, a hill 2883 feet high.

But between that hill and the rock on which the striæ appear,

there is the deep gorge of the *Coire Ruadh*, which if it then existed would have conducted any glacier from that hill in a different direction, viz., towards the N.W., and not towards Loch Creran, which lies almost due south from *Fraochaidh.*

3. Upon the south slopes of *Stob Coire Ruadh* there is a boulder of the peculiar *porcelain porphyry* worked at Kentallen in Appin. The boulder is about a square yard in size. That it is a boulder, is evident from the fact of the rocks of the hill where it lies being totally different. Its height above the sea is 2250 feet. Now, a porcelain rock of exactly the same kind occurs among the *Ben a Bheithir* hills, at exactly the same height above the sea, about midway between *Craig Ghorm* and *Sgorr Dhonuill*, which is 3 or 4 miles to the N.N.W.

Assuming that the boulder came from that point, it must have crossed two valleys, each of which is less than 700 feet above the sea. How it could have crossed these, except on floating ice, it is difficult to see.

4. There is another boulder among these hills deserving notice. It is one of *Schistose Breccia*, lying on the east side of *Fraochaidh*, at a height of 2235 feet. The rock of the hill here is a *Schistose Gneiss.* Now rocks of *Schistose Breccia* occur between the two peaks of *Ben a Bheithir* just mentioned, situated to the N.N.W. This boulder in like manner must have been carried across the deep valley of the Durer to have reached its present position.

5. The col between *Creran* and *Allt na Gaorran* showed glaciation coming down from the corries of the rough *Sgorr na Ulaidh*, and out of a corry on *Ben Fhionnlaidh.* Many loose and angular blocks of the hills themselves, much confusion, and smashing of every kind, and the glaciated contours, twisting away to go down both glens in opposite directions, S.E. and S.W., is all that this locality discloses. The deep cut of *Glen Ure* showed evidence of movement down it.

In reviewing the information obtained by me regarding these Creran boulders, I feel that there ought to be farther study of them, before their mode of transport can be said to have been discovered. On the one hand, the clustered manner in which the boulders lie on the west of *Miall an Aodain*, and at two spots on the east side of Glen Creran, is suggestive of blocks having rolled over the terminal

front of glaciers; or perhaps of a lateral moraine, when regard is had to there being in some places a train of blocks in almost single file. But, on the other hand, I cannot shut my eyes to the possibility of these boulders having been carried or pushed into position by ice in another form, which came from the west through Glen Tarbert; and which, when it reached the Durer valley, was blocked by the huge masses of *Scuir na Ulaidh* and *Ben Fionnlaidh*, and then forced to sweep down the trench of *Glen Creran*, carrying boulders, and lodging them where they now lie.

District of Glencoe.—On the western grass clad slopes of *Sron Coire Odhar Beg*, a hill north of *Glen Coe*, in the higher part of the glen, a number of small boulders, much rounded, were observed of a peculiar granite. It was whiter and coarser grained than the well-known Ardshiel granite, and had a little hornblende in it.

They were in composition altogether different from the rocks of the hill on which they were first noticed, which consists of schistose breccia.

The hills to the eastward I had previously examined (*Ben a Chrulaiste* and others), and knew that they consisted of epidotic gneiss.

I therefore thought it probable that the birthplace of the boulders would be somewhere to the westward, so in that direction I proceeded.

On reaching the *Aonach-Eagach* range, I found the same boulders, fewer in numbers but markedly larger in size.

They were lying almost exclusively on the eastern side of the narrow ridge leading up to the summit, and almost on the summit of the nameless peak marked 2938 feet on the 1-inch Ordnance map. On the next rounded haunch (2880 feet) they were not seen; but they reappeared on the ridge as it ascended to the eastern peak of *Meall Dearg* (3090 feet), and almost up to the summit of the western peak (3118 feet).

Their position here was most peculiar. They lay upon a ridge not many times wider than their own bulk, and only on the eastern slopes of that ridge; while on the lower hills where they were first seen, the same boulders lay on the west slopes.

The parts between *Meall Dearg* and *Meall Garbh*, extending to about half a mile, are quite inaccessible, and could not be examined. But so far as the peaked rocks composing this district could be seen, no

boulders were on them, and, indeed, on account of their sharp-edged ridges boulders were not likely to have lodged on them.

On the hills of *Sgornan Fiannaidh* (3188 feet) and *Sgor an Caiche* (2430 feet), situated farther west, these boulders were not found, nor any rock of the same description.

I proceeded to the next hills, of somewhat greater height, about 6 or 7 miles to the west, to the south of Balachulish, viz., *Bhein Bahn, Sgorr Dherag, Sgor Dhonuill*, and *Creag Ghorm.*

In the bed of a stream which descends the steep eastern face of *Creag Ghorm*, at about 1500 feet above the sea, a belt of rock occurs identical with that of the boulders; also along a great part of the semicircular ridge which connects *Creag Ghorm* with *Sgorr Dhonuill*, at a height averaging 2250 feet, there is rock very similar to that of the boulders, there being rather less mica in it, and only occasional hornblendic crystals. *Biddian nam Bian* (3786 feet) was twice ascended, but it presented no trace of the rock sought for. But though the rocks at the two other places indicated were found to be almost identical in mineralogical composition with that of the boulders, I am not satisfied that they supplied the boulders. The spots where these rocks occur are only from 1500 to 2300 feet above the sea; whereas the boulders on some parts of the *Aonach Eagach* to the eastward were at a height of 3100 feet above the sea.

Therefore I admit that there must still remain some uncertainty as to the birthplace of these boulders. An attempt has been made by some geologists to explain how boulders may be transported to positions above the level of the parent rocks; and if that theory be correct it may overcome the difficulty referred to.

It is possible also that the rocks at *Creag Ghorm* and *Sgorr Dhonuill* may have formerly reached a higher level; and in that view it may be remarked that at present the rocks of these hills are even now, under the action of the weather, breaking off into huge blocks.

Of course it may still be possible to find the peculiar rock of these boulders on more elevated hills elsewhere. Ben Cruachan and other hills to the south and west reach a height of more than 3100 feet; but I have been on most of these hills, and I do not think that on any of them there are rocks which would produce the boulders.

It is therefore a fact of considerable importance bearing on any theory of transport, that these boulders on *Aonach Eagach* occupy positions much higher in level than any of the hills in a very wide extent of country; so that it is difficult, if not impossible, to adopt for them the explanation of any local glacier.

I have adverted to the peculiar position of the boulders on *Meall Dearg*, where at a height of 3100 feet they lay upon a ridge not many times wider than their own bulk, or rather on the sides of that ridge facing the E. or N.E. I am not able to offer any satisfactory explanation of this feature. I would like again to study the positions of these boulders. They must have been brought there by ice, which may have come from the N.W., and stuck there among the high peaks till it melted, and allowed the boulders to subside on or near the top of the ridge. My explorations about Glen Creran led to the supposition of a flow of ice through Glen Tarbert on the N.W. side of Linnhe Loch. This might possibly also account for the boulders on *Aonach Eagach*. But in that case, where could the parent rocks be?

(Though it does not seem to have any direct bearing upon the question, yet it may be well to record the fact that the bed of the Cona is, for a short distance, about midway between the little lake and the hamlet of Clachach, cut through a rock very similar to, if not identical with, that of the boulders.)

III. *NOTES BY WILLIAM JOLLY, Esq.*

On the Carried Boulders on the South Shores of the Moray Firth.

In answer to your request, I send some notes, supplementary to those of last year, on the above subject.

The Dirriemore Granite seems to be more widely distributed towards the east than I anticipated. Since last year, I visited the place where I had formerly found it *in situ*, on the road between Dingwall and Ullapool, where it appears in the valley of the Blackwater, about and below its junction with Strathvaich. *None of it*

has been carried *westwards* between this part of the valley and Loch Broom, a tract which I have examined more than once. It has been carried altogether *towards the east*, in accordance with the general slope of the country. This granite would seem, however, to occupy a wider and more elevated area in the Ben Wyvis mountains than is shown in the Blackwater, from which it has been borne and dropped along the south shores of the Moray Firth, after being carried down the several valleys that drain this range into the Cromarty Firth, as well as down through Strathpeffer, and down the lower valley of the Conon below its junction with the Blackwater near Tor Achilty, in Contin.

In the valley of the Alness, for example, it is widely distributed, having evidently come from some centre near its head waters. Good specimens of it may be seen round the village of Alness, and along the shore between it and Invergordon, skirted by the public highway. It has been carried across the Cromarty Firth, and scattered abundantly in large and striking masses *over the whole of the Black Isle*, from end to end. Good examples of it may be seen at its northern extremity round Cromarty, and along its central ridge on the road between that town and Fortrose, large pieces being easily seen on the moor near Peddieston, a few miles south of Cromarty, and along the road between Invergordon Ferry and the Sutors. It is also found extensively along the whole of the east shore of the Black Isle, and has been carried thence eastwards towards Buckie. It exists plentifully all over the Laigh of Moray, and may be well seen along the seashore there, especially between Burghead and Lossiemouth.

The Stratherrick Liver-coloured Conglomerate I have found numerous additional examples of, from its source on the east shore of Loch Ness north of Inverfarigaig, onwards to Lossiemouth.

There would seem, however, to be two varieties of conglomerate distributed throughout the Laigh of Moray—the above easily distinguished rock, and another consisting of more angular components and entirely without the liver-coloured quartzite or porphyry. Examples of the latter may be seen in the old quarry of Oolitic limestone at the classical Linksfield, near Elgin, embedded in the boulder-clay there, one of the masses on the south side of the quarry being very large. The Douping Stone on the top of the Califer Hill, east of Forres,

mentioned in my notes of last year, is certainly of the Stratherrick liver-coloured variety; but the block on the top of Roseisle Hill, also mentioned by me, may be of the other. This second conglomerate would seem, from various indications, to have been transported at an earlier period than the Stratherrick, for it is found embedded at greater or less depths in the prevalent boulder-clay of Morayshire; whereas the Stratherrick rock is seldom, if ever, thus buried, being confined more to the upper surface of the country. The glaciation of Morayshire shows two main directions of the scratches, indicating two lines of ice movement from the westward, as exhibited admirably on the ridge of Carden Moor, near Alves station. These scratches point respectively 13° N. of W., and 6° S. of W., as the directions from which the ice has come. The second conglomerate may have been carried across the Moray Firth from Ross-shire, like the Dirriemore granite, in the line of the former scratches.

The red orthoclase Kinsteary Granite, found *in situ* near Nairn, is very abundantly distributed from this point towards the east, onwards beyond Buckie.

Mr Linn of the Geological Survey, at present engaged in mapping the district round Elgin, has found these three rocks widely spread all over the Laigh of Moray, and has taken the fullest notes of the composition and positions of the various carried blocks there, which will be embodied in his map of the region, and will form an important contribution to the question of the transportation of rocks along the south shores of the Moray Firth.

I append some notes supplied to me by Mr Wallace of the High School, Inverness, mentioned in last year's notes, regarding their distribution on the north coast of Banffshire. These carry the account of the transport of boulders eastwards to Cullen. It would be most desirable that the Committee should, if possible, obtain information regarding their farther distribution through Aberdeenshire, and thus complete their story to the German Ocean.

IV. *NOTES BY THOMAS D. WALLACE, Esq.*

On the Carried Boulders in the Parishes of Enzie and Rathven, Banffshire.

Having revisited this district at Christmas 1879, and examined it more carefully than on former occasions, I found further proof of the eastern flow of the great ice-sheet that at one time traversed the whole of the southern shore of the Moray Firth. In the neighbourhood of the Enzie post-office, I found numerous boulders of the Dirriemore granite, none of them so large as those that were dug out during the excavations for the Buckie Harbour, and mentioned last year in the Committee's Report.

Numerous small boulders of the Elgin Cornstones lie scattered all over the lower part of the district. Several are to be seen in the Gollachy Burn, a little to the west of Buckie.

Conglomerate boulders are rather rare. Except the few remaining stones forming the "Stone Circle of Dryburn," near Portgordon, I found only one, about a quarter of a mile east from Dryburn.

A very characteristic specimen of Kinsteary Granite is seen close beside the harbour of Buckie. Smaller pieces may easily be picked up on the fields along the shore. A well-marked feature of the schists which underlie the Old Red Sandstone in this district, is the frequent occurrence of large veins of calc spar, quartz, and quartzites. Specimens of these are also numerous in the drift.

A fine specimen of Cairngorm (water-worn) was picked up by a labourer on the high ridge to the south of the district, locally known as the "Hill of Altmore." It measures 2 inches thick at the one end and 3 inches at the other. It is about $4\frac{1}{2}$ inches in breadth. This man, ignorant of its value, took it to Aberdeen and had it polished on both sides by some friend at the granite works. This has rendered it quite transparent, so that one can read with the greatest ease anything placed under it.

One section of Boulder-Clay is deserving of notice. It is in the wood of Pathhead, on the estate of Cairnfield, a little to the south of the Enzie post-office. It consists of a fine plastic clay of a dark bluish-black colour, overlaid by the well-known red boulder-clay. The blue clay represents the denudation of the schists, and the red that of the Old Red Sandstone. Notwithstanding a very minute

examination of every burn in the district, I failed to find any of the blue clay on the lower ground. This I take to be an additional proof of the easterly flow of the ice.

As far as the boulder evidence in this district goes, it proves conclusively that the ice-flow was from the W., or a little to the S. of W.

All along the south shore of the Moray Firth there are scattered boulders of conglomerate, hornblende, and dirriemore (besides other) granites. In the neighbourhood of Inverness, these would indicate a drift from the N.N.W. and one from the S.W., both tending E., or a little to the N. of E. The boulders of hornblende might have come from the N.W. The only place where I have seen hornblende *in situ* in the neighbourhood of Inverness, near which are found numerous boulders of that rock, is at Raven's-Rock near Strathpeffer.

(Signed) DAVID MILNE HOME, *Convener.*

Mr Milne Home, Chairman of the Boulder Committee, after presenting the preceding Report, made the following remarks:—

I may explain that, to save the time of the meeting, and also to afford to members information regarding the operations of the Committee during the past year, copies of the Report were circulated with the billets for the present meeting;—and for the same purpose, as Convener of the Committee, I now proceed to give an abstract of the chief features of the Report.

I. *Boulders in Nairn, Moray, and Banffshire.*

I begin by alluding to the boulders in these counties, because the notes applicable to them are in some sense a continuation of the part of last year's Report applicable to these counties.

In these counties there are two classes of important boulders,—*Granites* and *Conglomerates.*

Of granites there are four kinds, distinguishable by the ingredients, and by the different districts where their parent rocks are situated.

There are, *First*, the boulders, consisting of a very peculiar granite, with lenticular pieces of dark mica, arranged in pretty regular layers, through a pinkish mass, giving to it some resemblance to a stratified deposit. The granite of these boulders has been identified by Mr

Jolly of Inverness with the granite rocks of the Dirrie Muir, a tract in Ross-shire situated to the west of Ben Wyvis, and lying about half-way between the east and west coasts. These boulders have been transported in a E.S.E. direction across the Cromarty Firth, over the district of the Black Isle, and across the Moray Firth, into the low grounds of Moray and Banff. The distance travelled must be nearly 100 miles. *Second*, there are two other granites, one red and the other grey, which have been transported from the hills forming the sides of the great Caledonian Valley,—called the Loch Ness granite and the Stratherrick granite.

These boulders also are found in Moray- and Banff-shires, and show a line of transport not quite the same as the Dirrie Muir granite, viz., about E. by N.

Mr Jolly says, that the Stratherrick granite boulders have been seen by him on the hills south of the Great Valley, up to a height of 1500 feet. But the Boulder Committee, three or four years ago, received through Captain White of the Ordnance Survey, notice of these boulders, having been found by his surveyors at heights of 2250 feet, the parent rocks being on hills 2900 feet in height. This fact is mentioned in the Committee's second annual Report.

These boulders, before reaching Morayshire, must have travelled also about 100 miles.

A fourth class of granite boulders in Morayshire and Banffshire is a beautifully pink-coloured rock, quarried at a place called Kinsteary in Nairnshire. No boulders of this peculiar granite are seen *east* of the parent rock.

What has now been said of *Granite* boulders, as regards transport, applies to the *boulders of Conglomerate.* There are two kinds of conglomerate rock forming them, and they come from different districts,—one in the Great Valley itself, which it crosses near the hill called Meal Fourvounie; the other in Ross-shire, at some distance to the north of the Great Valley.

The Committee have, in regard to these Moray- and Banff-shire boulders, obtained valuable notes from Mr Jolly and Mr Wallace, both resident in Inverness. The information given as to the position of the parent rocks is gratifying in this respect, that when, three years ago, many of these boulders were examined by myself, I drew an inference regarding the quarter from which they

had probably come, founded solely on the position and attitude of the boulders themselves, the correctness of which inference has now been confirmed by the discovery of the particular districts where the parent rocks are situated.

II. *Professor Forster Heddle's Explorations.*

The Professor's survey last year began on the West of Scotland, and extended from Ayrshire to Loch Torridon in Argyleshire; and also into the interior, near the districts called the Black Mount and Glencoe.

I was especially glad, on receiving the Professor's notes, to find that he had visited several of the islands of the Hebrides; because, as was explained in our last year's Report, the problem of the mode of transport becomes less complex on islands where there are neither hills nor valleys suitable for the formation of local glaciers.

1. The first island visited was *Colonsay*,—on which, however, nothing seems to have been found, beyond rock striations running W.N.W. and E.S.E., but which way the movement was, did not appear.

2. The next island was *Uist.* There, in like manner, the rock striations were W.N.W. and E.S.E., and it was there seen that the striating agent come from the westward. The Professor adds, that "the hollows or trenches between the higher grounds and the strike of the old gneiss strata have exercised some influence, in diverting the smoothing agent, sometimes one way and sometimes another."

At Loch Maddy in Uist, Professor Heddle met with Mr Harvey Brown, who had been for some time surveying there for objects of natural history, in company with Mr James Thomson, a member of the Glasgow Geological Society. Both of these gentlemen had also been studying the phenomena of boulders and striated rocks in the north part of Uist. Mr Brown supplied Professor Heddle with a note of the size of several boulders (which are specified in this Report), and he recommended the Professor to write to Mr Thompson for farther information. The Professor did so, and the answer he received from Mr Thompson was, "That the glaciation on the west shore of the Long Island was all from the west, varying occasionally between N.W. and S.W.;" and he "added (the Professor says) an expres-

sion of surprise, that any one could have made the mistake of not seeing this fact, it was so palpably evident."

3. In *Harris*, Professor Heddle found that on the southern flanks of the hill called *Gilebhall Glass*, rocks were striated up to a height of 500 feet, apparently by ice which (he says) came through the gorge of Tarbert from the west.

At this gorge, he found in the Till, boulders of a close-grained hornblende rock, doubtless (as he says) portions of a rock of identical character situated a few hundred yards to the westward.

The Professor adds, that as this crystalline rock is very marked and unmistakable, he regarded it as unimpeachable evidence of the course of the ice through the gorge.

4. *Scalpa* was next visited,—an island half a mile or so off the east coast of Harris. Here a granite boulder was "found *butted up* on its east side against the rocky steps of a knoll of gneiss rocks." A sketch of this boulder is given in the Report. With reference to the direction of transport, Professor Heddle mentions that the same rock of which the boulder is composed forms a bed in a high cliff on the mainland of Harris to the N.W.

5. *The Shiant* islands, which were next examined, are three in number. They also are off the east coast of Harris, about twenty miles to the N.E. of Tarbert.

On the surface of these islands, which are of basalt, no foreign erratics of any size were found. But on the shore of two of these islands, he found blocks of conglomerate and Cambrian sandstone, which had probably come from near Stornoway, about 30 miles to the north, where these rocks are *in situ*.

The Professor saw in the two largest islands, which lie N. and S. of each other, that some agent—it might be ice—had passed over them from the west, smoothing them, and pushing fragments of the trap rock towards the east. But he observed particularly that there was no such smoothing on the third island lying to the east; and the only explanation of this fact which occurred to him was, that the third island, being much lower in level than the other two, the ice may have passed over, without touching it—an explanation suggesting the agency of floating ice.

6. The next island visited was *Skye*, but the Professor was able only to examine the N. and N.E. portions, viz., from Aird Point to Portree.

He was surprised to find no boulders either on the coast or on the hills adjoining the coast, except on the small islet of *Stainchol*, at the mouth of Loch Staffin. On the shore of this loch there were blocks of Cambrian sandstone, a rock of which he had found pebbles on the shore of the Shiant Islands. On Stainchol, he also found a boulder of dolorite, containing much labradorite;—a rock of the same nature, was *in situ* about 50 yards to the N.N.W.

Professor Heddle further attests, that the rocks on the hills examined by him, which he ascended to above 1500 feet, "nowhere bore groovings or even scratchings;" and he states that "the *cols* between the numerous heights were narrowly examined by him."

Now these facts seem to have an important bearing on the question of boulder transport. Mr James Geikie, in his "Great Ice Age," p. 77, says, that "most of the islands which lie off the coasts of Scotland plainly indicate, by striations and other glacial markings, that ice has swept over them." He adds that "The most striking example of this is furnished by Lewis, the northern portion of the Long Island, which (says he) I found to be glaciated across its whole breadth *from* S.E. to N.W. The land-ice that swept over this tract, must have come from the *mountains of Ross-shire*—a distance of not less than 30 miles. Leaving the mainland, it must have filled up the whole of the North Minch (60 fathoms in depth), and overflowed Lewis to a height of 1300 feet at least."

This statement, made in 1877, was repeated in two elaborate papers read before the London Geological Society in 1878, in which it was maintained, "that the whole of the Long Island, from the Butt of Lewis to Barra Head, has been overflowed from the Minch by ice that moved outwards from the inner islands and the mainland."

Now this theory seems entirely at variance with the facts ascertained by Professor Heddle last year, and by myself in the previous year. If a mass of ice came from the Ross-shire hills, so great as to fill the Minch, overflow the Long Island to the height of 1300 feet, and to stretch from the Butt of Lewis to Barra Head, a distance of about 80 miles, it must have impinged on the island of *Skye*, and especially on the north-east part of it. But there, according to Professor Heddle, no boulders are to be seen, and even no groovings or striations of the rocks.

On the other hand, boulders and striated rocks, which in the

Long Island are plentiful, all indicate a movement from the N.W. —a direction the very opposite of that requisite for a great glacier from Ross-shire.

Mr Geikie, in a footnote (page 60), says that "Mr Campbell of Islay considered that the Hebrides Islands had been glaciated by sea-ice coming from the N.W. ;—while, on the other hand, my observations in Lewis compelled me to believe, that the glaciating agent was land-ice streaming outwards from the mainland. My colleague, Mr Etheridge, Jun., who accompanied me during my last visit to the Long Island, also concluded, that the glaciations had been effected by land-ice coming from the S.E."

Whilst much weight is proper to be given to the observations and opinions of such experienced geologists as Mr James Geikie and Mr Etheridge, on the other hand it is only right to keep in view that the late Robert Chambers and Dr Bryce, though they wrote no papers on the subject, are known to have concurred with Mr Campbell; and I have reason to believe, that Mr Jolly of Inverness is of the same opinion.

7. *Black Mount District.*

(1.) Some white granite boulders, which were noticed by Professor Heddle on the shores of Loch Tulla, were, after a minute and laborious search, traced by him to a hill called *Albannach*, situated about 10 miles to the W.N.W. Professor Heddle having ascertained the line of transport, next tried to find whether the boulders covered a large space transversely. The result of this search was to show that (to use the Professor's words) "these boulders had been carried, as it were, in a stream, and one of no great width, towards the S.E."

(2.) Notice was next taken of several large boulders, one weighing no less than 1900 tons, in a valley, which becomes very narrow to the east of Loch Dochard. The Professor says, "If any powerful agent passed through this valley, there would be great obstruction and a violent pressure on and rending of the adjoining rocks. The lower part of the pass (he says) contains much till; and occasionally rock rises up through the till with finely-smoothed hunches, showing striations from the W.N.W." In reference to the large boulder above referred to, Professor Heddle gives his opinion that "nothing but ice could have brought it into its present position."

(3.) The *Glen Creran* boulders having been referred to in the two last Reports of the Committee, with a confession of uncertainty as to the source from which they had come, Professor Heddle, in compliance with my request, kindly undertook a renewed survey of the district.

The result has been that the Professor has found rocks on the hills 4 or 5 miles from Glen Creran to the N.N.E. identical in composition with the boulders. But how the boulders were carried from these hills, where the parent rocks are from 1700 to 2000 feet above the sea, to Glen Creran, which is only about 200 feet above the sea —*i.e.*, "whether dropped from floating ice, or carried by glaciers," "it is (observes the Professor), with our present information, impossible to say."

He found on the hills containing the rocks of the boulders numerous *striæ*, which showed "that some powerful smoothing and striating agent had passed over this district from the west, and at a level exceeding 2000 feet above the sea. But west from the place where these smoothed and striated rocks occur, there are no hills so high as to produce a glacier, unless, indeed, a glacier had come through Glen Tarbert, which is a continuation of Loch Sunart, and crossed what is now the Linnhe Loch. Loch Sunart and Glen Tarbert occupy a hollow in the district which runs in a direction about W.N.W. and E.S.E.

"It is, however (he says), proper to add that on the rock where these W.N.W. striæ occur, there are cross striæ overlying and cutting into these, indicating another and more recent agency from the S.W. These cross striæ being more sharp and minute than the first, indicate more recent and less powerful action. Can it have been that a sea existed exceeding 2000 feet above the present level with ice in it, which was floating about in eddying currents among what are now high-peaked hills, tearing rocks out of the shallows, and pushing them over what were then submarine cliffs?"

(4.) In this part of his notes applicable to the Glen Creran district, Professor Heddle refers to what he calls "*a boulder of the peculiar porcelain porphyry* worked at Kentallen in Appin." That it is a boulder is evident from the fact of the rocks of the hill where it lies, being totally different. Its height above the sea is 2250 feet. Now, porcelain rock of the same kind occurs among the *Ben a*

Bheither hills at exactly the same height above the sea, about midway between two other hills whose names are given, 3 or 4 miles to the N.N.W.

Assuming that the boulder came from that point, it must have crossed two valleys, each of which is less than 700 feet above the sea. How it could have crossed these, except on floating ice, it is difficult to see.

(5.) There is another boulder in the same district of *Schistose Breccia* at a height of 2235 feet. The parent rock was found at some distance to the N.N.W. "This boulder (the Professor says) in like manner must have been carried across the deep valley of the Durer to have reached its present position."

(6.) A very interesting account is given of boulders in the neighbourhood of *Glencoe.* Being much rounded, they suggest a long transport, and were "of a peculiar granite," somewhat like "the well-known Ardshiel granite," only "whiter and coarser grained."

Being "altogether different from the rocks of the hill on which they were first noticed, consisting of a 'schistose breccia,' the Professor resolved to seek for the parent rock."

Thinking, from his knowledge of the rocks to the *eastward*, that they were not likely to have come from that quarter, he set out on a hunt in a westerly direction. On reaching the *Aonach-Eagach* range of hills, he recognised the same boulders on them, "fewer in number, but markedly larger in size."

He followed them up to the first summit of the hill, which was 2938 feet; and, proceeding still further west to a hill called *Meall Dearg*, he found the same boulders first at 3090 feet and eventually "almost up to the summit of the western peak at 3118 feet."

The Professor says that "their position here was most peculiar,—they lay upon a ridge not many times wider than their own bulk, and only on the eastern slopes of that ridge."

Proceeding still farther west to other hills (which are named in his notes) at from 2400 to 3200 feet, the Professor did not find either boulders, "or rock, of the same description;" but on proceeding to the next hills, of somewhat greater height, about 6 or 7 miles to the west, he found at two spots, the kind of rock he was in quest of. He however adds, that "though the rocks at these two spots were almost identical in mineral composition with that of the

boulders, I am not satisfied that they supplied the boulders,—for the spots where those rocks occur, are only from 1500 to 2300 feet above the sea,—whereas the boulders on some parts of the *Aonach-Eagach*, to the eastward, were at a height of 3100 feet above the sea.

"An attempt has been made by some geologists to explain, how boulders may be transported to positions above the level of the parent rocks; and, if that theory be correct, it may help to overcome this difficulty."

"But it is a fact of considerable importance, bearing on any theory of transport, that these boulders on *Aonach-Eagach*, occupy *positions much higher in level than any of the hills in a very wide extent of country*, so that it is hardly possible to adopt for them the explanation of any local glacier."

"I have adverted" (says the Professor) "to the peculiar position of these boulders on *Meall Dearg*, where, at a height of 3100 feet, they lay upon a ridge not many times wider than their own bulk, or rather on the sides of that ridge facing the E. or N.E. I am not able at present to offer any explanations of this feature. I would like again to study the position of these boulders. They *must have been brought by ice*, which may have come from the N.W. and stuck there among the high peaks, till it melted and allowed the boulders to subside on or near the top of the ridge. My explorations about Glen Creran, led to the supposition of a flow of ice through Glen Tarbert on the N.W. side of the Linnhe Loch. This might also possibly account for the boulders on *Aonach-Eagach;* but, in that case, where could the parent rocks be?"

This query by the Professor, Where could the parent rocks of these boulders be? he leaves unanswered; and, no doubt, it is a query more easily asked than answered. It would, therefore, be presumption in me even to suggest an answer. But the query reminds me that, two years ago, I sent specimens of the Loch Creran boulders to Professor Judd of London, an eminent geologist well acquainted with the rocks of the West Highlands, to ask him, whether he knew of rocks anywhere like those of the boulders, and he gave a decided opinion that rocks of exactly the same kind existed in Mull and Ardnamurchan. Now, these places are to the west of the boulders referred to by Professor Heddle, and

it is from the west that he thinks they came. Moreover, in Mull, the hill of Benmore is 3180 feet above the sea, whilst in Ardnamurchan there are hills nearly that height. It strikes me, therefore, that it would be very desirable, if Professor Heddle could, in the course of this summer, visit Mull and Ardnamurchan to see whether he agrees with Professor Judd's surmise on this subject.

III. *Convener's Notes.*

The points brought out in these, are very unimportant, compared with those of Professor Heddle and Messrs Jolly and Wallace of Inverness.

1. The boulders in *Cantyre* I found had, on the south and east coast, come apparently from some point due north; those on the west coast, from points varying between N.W. and N.N.W.

2. In *Arran*, the boulders on the east coast, which were all that I examined, seem to have moved in a direction from about due north.

3. In the *Cumbrae* islands, they seemed also to have come from due north.

4. In *Loch Long* and the *Gairloch*, the boulders showed transport from points varying between N.N.W. and N. by E., which happens also to be about the axial line of the valleys in which the boulders lie.

5. In the hills to the north of *Loch Fyne*, I was rather surprised to see the smoothed rocks facing N. and N.E., and the boulders lying with their longer axis in much the same direction.

6. When I reached *Loch Awe*, I found the boulders among the hills, at from 900 to 1000 feet above the sea, indicating in like manner transport from the N.N.E.

This deviation, at several places in the interior of the country, from the N.W. direction which is so prevalent elsewhere, at first rather surprised me; but it probably does not on principle differ materially from the fact, that occasionally on the same rock, or on the same boulder, there are separate sets of striæ. If these striæ are produced by currents which run first in one direction and thereafter in another, a similar explanation might apply to the variations of direction over a large district of country.

For example, Professor Heddle, as we have seen, takes notice of

such variation in *Uist*, and on a larger scale among the hills at Glen Creran; and the boulders in Nairn and Morayshire have evidently been brought by currents which came from different points.

If in the North of Scotland, the normal direction of the current was to the S.E., it is probable that the deep trench of the Great Caledonian Valley running about E. by N, with a range of hills on each side 2000 feet high, would there cause a deviation in the direction of the current. As the sea subsided from one level to another, the currents would change in directions.

Examples were seen by me last year in the Lewis, of a change even on the same hill. At the top, the direction was as usual N.W., near the bottom, it was from due W. or W.S.W.

Among the hills south of Loch Awe, I found a large boulder perched on a peak of rock in a remarkably precarious position. It is shown on the diagram. By a glacier it certainly could not have been brought, there being neither hills nor valleys to form a glacier. If it came by floating ice, the ice might be arrested by the peak, and when it melted, the block which the ice carried, might remain.

7. The largest boulder which I have yet seen, was found by me on the west coast of Argyle, in Loch Killasport. Calculating by its cubical contents, it weighed about 2770 tons. This boulder, and many others of large size, were on the sea shore, and half a mile at least from any sea cliff, old or recent. I felt convinced from their situation, and also from the direction of their longer axis, that they had all come across the sea from the N.W.

8. A short time ago, my attention was called to a boulder, $9 \times 8 \times 6$ feet, in Roxburghshire, weighing about 16 tons. On examining it, I found that it was of exactly the same rock as that which composes the Penielheugh, the hill on which the Waterloo Pillar stands. It is about a mile to the east of the hill, and has evidently been floated to its present position by ice. The hill also presents other facts of no small interest bearing on the transport of boulders. The west side of the hill has been swept bare, so that the trap rocks stand out like the bones of a skeleton with the skin and flesh off, whilst the east side of the hill is covered by soft Old Red Sandstone, as well as by sand and gravel. This place affords undoubted evidence of sea with floating ice, which stripped the hill and carried fragments to the eastward.

Whilst the view I take in regard to the transport of boulders, and the striation of rock surfaces in Scotland is, that these phenomena were in most instances due to ice in a sea, which reached to our highest mountain tops, I admit that there are traces also of land ice in the form of local glaciers. In last year's Report I pointed out what appeared to me clear evidence of glacier action in Glencoe; and Professor Heddle also recognised glacier action on the west coast near Loch Torridon. But my idea is, that these glaciers must be referred to a period antecedent to the submergence of the land, for we find those traces of glaciers in many places covered over by thick beds of gravel, sand, and clay which could only have been deposited by the sea.

DAVID MILNE HOME, *Convener.*

On 21st May 1880, at a meeting of the Council of the Society, the Committee was reappointed, with the addition of General Bayley and Professor Duns, D.D.

5. On Two Masks and a Skull from Islands near New Guinea. By Professor Turner.

These specimens have recently been presented to the Anatomical Museum of the University, by J. Wharton Cox, Esq., who had received them from his father, Dr Cox of Sydney, the well-known Australian naturalist.

The masks had been procured by Dr Cox from missionaries, and were either from the island of New Ireland or New Britain, in proximity to the north coast of New Guinea. They were both formed of the frontal and facial bones, on which a face had been modelled in a composition, formed of a mixture of a resinous substance with earth or clay. This artificial face had then been painted with red, black, and white pigments. The larger mask was hollowed out behind, by the removal of the sphenoid and ethmoid bones, so that it could be adapted to the face of a wearer, and a bar of wood was fastened transversely across the hollow, which the wearer had evidently used for holding the mask between his teeth; as the mask had both the eyelids and lips

separated from each other, the wearer could both see and breathe through these openings.

The smaller mask was not capable of being closely adapted to the face of a wearer, for the sphenoid and ethmoid bones were in position, and the orbits were filled up with a composition similar to that employed in modelling the face; and an artificial eye formed of the operculum of the shell of a mollusk was fixed in each orbit. An artificial tongue formed of a piece of bright red cloth folded on a wooden framework partially projected through the open mouth. Whiskers and beard, which in the larger mask were modelled in the hard composition, were in the smaller mask formed of vegetable fibre, and from their mode of arrangement gave a pantaloon-like character to the face. This mask may have been used as an ornament, or as an object of worship.

Warrior Island, from which the skull was procured, is an island off the south coast of New Guinea, and to the north of Torres Straits. The skull was smeared on the forehead and face with streaks of red pigment. The orbits were filled up with a hardened material, to which lozenge-shaped pieces of mother-of-pearl to simulate eyes were attached. A plug of wood $3\frac{1}{4}$ inches long, cut so as to represent an artificial nose, was inserted into the anterior nares.

The skull was that of an adult man, 175 mm. long, 154 broad, 137 high, 515 in horizontal circumference, and with a capacity of 1650 cubic centimetres. It was brachycephalic, megacephalic, mesorhine, and mesognathous.

The skull was compared with the crania of Australians and Papuans, which are dolicocephalic, microcephalic, and prognathous; and it was pointed out that its affinities were not with these races but with the Malays.

Various methods of decorating preserved heads and skulls were then referred to. This communication will appear *in extenso* in the "Journal of Anatomy and Physiology," July 1880.

6. On an Ultra-Neptunian Planet. By Professor G. Forbes.

In continuation of researches communicated to the Royal Society of Edinburgh, 1880 (February 16), in which I gave the

probable position of an ultra-Neptunian planet, I have now to inform the Society that I have detected the existence of perturbations in the motion of Uranus, agreeing remarkably in character and period with those which would be produced by the new planet. These results are obtained from observations of Uranus extending over more than a century. The position of the planet, from this point of view, is found, from the first rough examination, to be the same as that given by me in my former memoir. This gives a means of determining the mass of the new planet. In this way I find it to be about the same as that of Saturn. I have also some reasons for believing that the following stars observed by Rümker, but stated by E. J. Cooper ("Markree Catalogue of Stars," vol. iv. p. 229) to be missing, are actually the new planet.

Number.	R.A.	N. Decl.
3320	10h. 37m. 24s. 203	10° 50′ 57″ 21
In Nach.	10 38 47 179	10 46
3372	10 44 24 365	9 55 52 58

I have not Rümker's Catalogue at hand at this moment to identify them.

The following star in Bessel's zones is also missing, and may be an observation of the planet. Cooper declares it to be missing.

Mag.	Zone.	R.A.	N. Decl.
9	280	9h. 51m. 59s. 82	16° 45′ 12″ 5

Monday, 7th June 1880.

J. H. BALFOUR, M.D., in the Chair.

The Chairman presented the Keith Medal for the Biennial period 1877–79, to Professor Fleeming Jenkin, for his Paper "On the Application of Graphic Methods to the Determination of the Efficiency of Machinery," the second part of which was published in the Society's Transactions for 1878, and in doing so made the following remarks:—

Professor Jenkin has contributed several valuable papers to our Transactions.

In 1869 we had "The Practical Application of Reciprocal Figures to the Calculation of Strains on Framework," in which he exemplified in a very clear manner the mode of applying to important *statical* questions a beautiful principle, due in part to Rankine but mainly to Clerk-Maxwell.

The paper for which the award of the Keith Prize is now made is more thoroughly original, and may be roughly described as an extension of Maxwell's principle to the *kinetics* of machinery, where all parts move in one plane. It is entitled "The Application of Graphic Methods to the Determination of the Efficiency of Machinery." The first part was read to the Society in 1877, and the second in the following year. All three of these papers are in our Transactions.

Among his other contributions may be mentioned his application (in conjunction with Professor Ewing) of the Phonograph records to the "Harmonic Analysis of certain Vowel Sounds." This is an ingenious and elaborate piece of work, and shows us (among other things) within what wide limits the components of a sound may vary while it is still recognised by the ear as having a definite vowel quality.

Professor Jenkin, in handing you this medal I express, I am sure, the feelings of all the Fellows of the Society, when I say that we thank you heartily for the valuable contributions you have already sent to our Transactions, and that we look with confidence for an additional series.

Professor Jenkin then took the Chair.

The following communications were read:—

1. Non-Euclidean Geometry. By Professor Chrystal. (Plate XX.)

When I had the honour of being asked by the Council of the Royal Society to give the following address, I chose the subject partly because it had been brought under the notice of the fellows by my predecessor, Professor Kelland. His memoir was written comparatively early in the history of the subject; and he seems to have been but little acquainted with what others had done even up to the time at which

he wrote. Accordingly, although the subject is treated very ably in his paper, it is treated from only one point of view; and, indeed, one side of it is left out of sight altogether. The relation of the whole theory to the question of the origin and mutual independence of the axioms of geometry has been made much clearer of late, and I believed that some account of the more modern views might be of interest.

I am particularly desirous of bringing *pangeometrical* speculations under the notice of those engaged in the teaching of geometry. In discussing with schoolmasters the difficult problem of the reform of geometrical teaching, I have met with much enlightened and some unenlightened criticism. The former kind of criticism has convinced me that many teachers of mathematics will be glad to have this subject made more accessible; and I believe that a knowledge of what great mathematicians have thought on the subject would destroy criticism of the latter kind altogether.

It will not be supposed that I advocate the introduction of pangeometry as a school subject; it is for the teacher that I advocate such a study. It is a great mistake to suppose that it is sufficient for the teacher of an elementary subject to be just ahead of his pupils. No one can be a good elementary teacher who cannot handle his subject with the grasp of a master. Geometrical insight and wealth of geometrical ideas, either natural or acquired, are essential to a good teacher of geometry; and I know of no better way of cultivating them than by studying pangeometry.

The following sketch is addressed to those already familiar with Euclid's geometry. I have made no attempt to give a detailed account of modern researches, or to build up a systematic treatise. I have simply tried to give in a synthetic way a general idea of what is known in a certain department of a now very widely developed subject. In so doing I have used the materials and methods of Euclid as much as I consistently could, at some sacrifice of elegance, no doubt, but with obvious practical advantage.

I have not attempted to give any bibliographical details, for the simple reason that any one who wants them will find nearly all that can be desired in two papers by Mr Halsted in the first volume of the "American Journal of Mathematics."

On Pangeometry.

I know of no question possessing more interest for a thinker, and none of more importance for a mathematician, than the well-worn one of the origin of the axioms of geometry.

Passing over the discussions of mental philosophers, which, so far as I am acquainted with them, are of little mathematical or physical interest, we find two great modern contributions to this interesting subject; one by the mathematicians headed by Gauss, Lobatschewsky, Bolyai, and Riemann; the other by the physiologists represented by Helmholtz.

The mathematical investigators may be taken as representing the subjective side of the subject, the physiologists as representing the objective; although, in point of fact, Helmholtz, the personal representative of the latter, is a happy union of both classes of philosopher.

Any purely abstract science starts with certain data called definitions and axioms;* and of these materials reason builds the fabric of the science.

I do not intend to take up the question of the origin of axioms directly. On the contrary, I shall lay down axioms, and the only argument against me, so far, will be to prove the inconsistency of my conclusions with my premises, or with one another.

The absence of such inconsistency is what I mean by conceivability. I do not deny that other meanings may be attached to this word, and that the question of the conceivability of axioms might be profitably discussed from other points of view. We might discuss it as a purely personal question, each man to be judge and jury, or it might be granted, as I, for the most part in what follows, take it to be, that any axioms that can be made the foundation of a consistent reasoned system are given *à priori*. I suspect that this would be

* In Euclid's Geometry the functions of definition and axiom are not always clearly separated; at all events, some of his definitions serve purposes for which others are unfit, and this must be kept in view in what follows. With postulates I have at present nothing to do, as I am concerned solely with geometrical theorems. The *mixture* of problems with theorems is a peculiarity of Euclid's method for which there is no absolute necessity, and which is certainly inconvenient in an elementary text-book. Geometrical constructions are in a sense the applications of geometrical theory, and ought to be kept by themselves. The Society for the Improvement of Geometrical Knowledge have acted wisely, I think, in following this arrangement in their syllabus.

allowed by most of those who have considered the question of axioms in what I believe to be by far the most useful and effective way, viz., by examining and pushing the conclusions to be drawn from them to the utmost; and by investigating what change on these conclusions would be induced by varying one or more of the axioms themselves.

The question might also be approached from the side of experience. I take, for the sake of illustration, an instance which brings me at once to my subject. We have, by generalisation from experience, ideas more or less refined according to our individual physical education of a geometrical straight line, and of a geometrical point. Let us think, then, of two straight lines intersecting at a point, and let us ask ourselves, Can two such lines intersect again? Our first impulse is to answer no; but due consideration will show us that, in point of fact, experience does not settle the question. All we can say is that no one starting from the point of intersection of two straight lines has ever followed them by physical (say optical) observation to a second intersection. But then we must admit that, on our usual assumption that space is of infinite extent, and straight lines of infinite length, the distance through which any one has so followed them is, after all, relatively speaking, but an infinitely little way. Our assertion, therefore, that two straight lines never intersect again is merely an assumption, accordant, no doubt, with our limited experience, but otherwise unfounded, and certainly not of necessity involved in our idea of straightness, though we may superadd it thereto if we please. I recommend those who doubt this statement to begin by defining a straight line by a single geometrical property, which is not verbally equivalent to the assertion in question, and to attempt to prove it.

It may be well to remark here that the discussion of the properties of tridimensional space in reality divides itself into two parts:—first, what may the properties of space be conceived to be? *conceive* being understood in the sense above explained; second, what are the properties of space as we know, or think we know, them? The former question is a purely mathematical one; the latter is one in the main for the physicist or the mental philosopher, and the function of the mathematician in connection with it is to make clear what the question exactly is, and what alternatives are open for us. What the bearing

of modern mathematical research on this point appears to be, I shall endeavour to explain later on.

With these preliminary remarks in explanation, I now proceed briefly to sketch a system of geometry which, as to its foundations, differs from that of Euclid only in the alteration of one (or at most two) axioms. Its conclusions will be found to differ very materially from his, although this difference is merely in the way of wider generality, Euclid's geometry being contained as a particular case in what I shall, for distinction's sake, call Pangeometry.

The space which I shall consider is to be tridimensional. I appeal to the ordinary conceptions of

Point, Line or curve, Surface, Solid;

and, for the sake of the words, state that a point has no extension, a line is once extended, a surface twice, a solid thrice.

As a test of these distinctions, the idea of motion may be introduced. I cannot stop now to justify this, but merely remark that nothing is to be predicated concerning time.

Farther, space is to be uniform, in the double sense that it has no properties depending either on position or direction.

The great test of this last statement is congruency,* which I mention thus early, because it is the touchstone of geometry. Thus the statement that space has no properties depending on position, simply means that congruent figures exist, *e.g.*, that a solid of a certain size and shape can be carried from one part of space to another without alteration in either respect; and that two congruent figures can be conceived as separately existing in different parts of space. It is evident that all space measurement rests on congruency.

It is essential to be careful with our definition of a *straight line*, for it will be found that virtually the properties of the straight line determine the nature of space.

Our definition shall be that two points *in general* determine a straight line, or that in general a straight line cannot be made to pass through *three* given points.

It is important to notice the force of the phrase *in general*. This

* Two figures are said to be congruent when one can be placed on the other, so that every point of one shall coincide with a point of the other, and *vice versa*. The phrase *equal in every respect* is used in the same sense in most English editions of Euclid.

will be best understood from an illustration. We all know from the case of a three legged stool, if not from any more scientific source, that three points determine a plane. Yet not any three points; for, if the third foot were put in line with the other two, the one stool would be as unsafe a seat as the proverbial two. Yet again, and very near indeed to our case, two points on a sphere in general determine a great circle on it. But there are exceptions; a point and the diametrically opposite point do not determine a great circle, and yet it would be a good definition of a great circle to call it that line on a sphere which is in general determined when two of its points are given, no other condition being assigned.*

We recognise therefore that, although in general, any two points being taken, a line will thereby be determined, yet it may happen that, one point being taken, another point may exist which along with the first does not determine a straight line. The necessity for this admission appears when we consider space in which two straight lines have more than one point of intersection.

Here let it be mentioned, to avoid misconception, that it follows from our definition of a straight line, and from the uniformity of space (the test being congruency), that space is symmetrical round every straight line. This is at once an answer to those who say that pangeometry is merely an analogy drawn from the theory of surfaces of constant curvature.

A plane may be defined as Euclid defines it, and the conclusions drawn, that two intersecting lines, a point and a line, or a line passing through a given point and moving perpendicular to a given line, all in general determine a plane. The last form of definition of course presupposes the definition of a right angle.

Farther, we adopt all Euclid's definitions up to the definition of an

* It is interesting to notice that any curve already conditioned a number of times less by two than the whole number of conditions that completely determine it, fulfils in many respects the definition of a straight line, for any two points completely determine the curve. A very interesting particular case is that of a series of circles which always pass through a given fixed point. Such a series of circles may take the place of straight lines in many of Euclid's propositions. Most of the propositions as to congruency hold for them. The sum of the three angles of a triangle formed by three such circles is two right angles; the perpendiculars from the vertices of such a triangle on the opposite sides are concurrent; and so on, as is otherwise evident by the theory of inversion.

acute angled triangle, but reject in the meantime, at all events, all that follow in the first book.

Next we adopt Euclid's propositions concerning angles at a point, viz., I. 13, 14, 15; also the propositions as to congruency I. 4, 5, 6, 8, and the first part of 26, with a protest to the effect that in many cases his demonstrations are needlessly circuitous and difficult. All that is wanted for the demonstration of these propositions is the defining property of the straight line and the ordinary axioms and definitions as to equality.

Different Kinds of Space.

Before going farther, we must distinguish the different cases that may arise when we consider two intersecting straight lines.

1. They may never intersect again and be of infinite length (*i.e.*, each is non-re-entrant). Space which has this characteristic is called, for the present, hyperbolic space. We shall see, however, by and by that another case must be distinguished under this head, that, viz., of homaloidal or Euclidean space.

2. They may intersect again. Space having this characteristic is called elliptic space.

The simplest space of this kind is that in which a straight line returns into itself, so that the next point in which two straight lines intersect is the point in which they first intersected. In this kind of space, which I shall call single elliptic space, two straight lines intersect in only one point; and there is no exception to the statement that two points determine a straight line.

The next simplest case would be that in which two straight lines intersect a second time in a distinct point, and then re-enter at the next point of intersection which coincides with the original one. This might be called double elliptical space. I am not yet certain* whether the symmetry of space will allow us to carry this multiplicity

* I have not been able to find a definite settlement of this question by any of the great authorities on hyper space. Frischauf takes double elliptic space as the representative of elliptic space, and seems to hold that this is the only possible kind. Klein ("Mathematische Annalen," vi. 125) takes single elliptic space, and criticises Frischauf's view ("Fortschritte der Mathematik," viii. 313, 1876). Newcomb (Borchardt's Journ., lxxxiii. p. 293) professes himself unable to settle the question. If the notion of double elliptic space cannot be shown to be self-contradictory, then it would appear that the question becomes simply one of the choice of axioms. See note below, p. 661.

of elliptical space farther. In the meantime, I may remark that in a space of this second kind we must, as already explained, admit exceptions to the statement that two points determine a straight line.

In what follows I take single elliptical space as the representative of elliptical space generally, although on account of the non-existence of a closed surface of uniform positive curvature, on which a pair of geodetics intersect only once, the conclusions of the geometry of single elliptical space appear in some respects more bizarre than those of double elliptical space, whose planimetry is mirrored by the geodesy of a sphere.

It is obvious that Euclidean, or homaloidal, space is included in hyperbolic space as above defined. We shall afterwards show, however, that it may be regarded as a limiting case of elliptic space. It is therefore the transition case lying between the other two.

Sketch of the Geometry of Hyperbolic (Infinite) Space.

From the definition of this kind of space it is clearly infinite. Here I must insist on the distinction between infinite and unbounded, a distinction first brought into notice by Riemann. The uniformity of space necessarily involves the notion that it is unbounded, but by no means necessitates that it shall be infinite in extent; in fact, I shall point out directly that a single elliptical space is necessarily of finite extent.*

After the propositions relating to congruency already proved, the next fundamental proposition to be established is the following :—

In hyperbolic space the sum of the three angles of a rectilineal triangle cannot exceed two right angles.

The following proof of this proposition is due in substance to Bolyai. Legendre had given another, but he failed to see exactly the nature of the assumptions on which he founded.

ABC (fig. 1) is any triangle, O the middle point of BC, OD = OA; so that CD falls within the angle BCL. (Here we assume that a straight line is non-re-entrant, and that a pair of straight lines never intersect twice.) Then DOC ≃ †AOB; and ADC is equal in area to ABC, and

* An ellipse and a circle are unbounded but finite lines; a hyperbola is both unbounded and infinite.

† I adopt the sign ≃ used by continental writers for *congruent to*, or *equal in every respect to.*

has the sum of its angles the same, while the sum of A and D = BAC. Of these angles one is $\ngtr$, and the other $\nless$ than $\frac{1}{2}$ A. Taking the least of them, and bisecting the opposite side, we derive as before from ADC a triangle, still having the same area, and the same sum of all the angles, but in which the sum of two of the angles $\ngtr \frac{1}{2}$ A.

By a similar process we derive another triangle, still having the area and the sum of its angles unaltered, but in which the sum of two angles $\ngtr \frac{1}{2^2}$ A.

At last we get a triangle, in which the area is the same as at first, and the sum of the angles the same, but the sum of two of them $\ngtr \frac{1}{2^n}$ A, where n may be as great as we please; that is, in which the sum of two angles is as small as we please.

But the third angle can never be greater than 2R, hence the sum of the angles of the original triangle cannot be $>$ 2R.

It is to be noticed that this demonstration would fail if a straight line were re-entrant, or if two straight lines had more than one point of intersection.

Corollary.—If C′ be the external angle at C of the triangle ABC, then, since

$$A + B + C = 2R - \delta,$$

where R stands for a right angle, and δ is either zero or essentially positive, and

$$C + C' = 2R,$$

we have

$$C' = A + B + \delta;$$

That is, *the exterior angle of any triangle is not less than the sum of the two interior opposite angles.*

Of course it follows that *the exterior angle of any triangle is greater than either of the interior opposite angles; and that the sum of any two angles of a triangle is less than two right angles.*

We can now prove for hyperbolic space :—

That the greater side of every triangle has the greater angle opposite, and conversely.

That any two sides of a triangle are together greater than the third side.

Also *Euclid I.* 21.

Euclid I. 24 *and* 25.

Euclid I. 26 (*the second part*).

Also the usual propositions concerning the perpendicular and the obliques drawn from a given point to a given straight line.

The amount by which the sum of the three angles of a triangle falls short of 2R is called the *defect* of the triangle. This is the same as the excess of the sum of its exterior angles over 4R. If we take the latter statement of the definition, we may talk of the defect of any plane rectilineal figure. In forming the external angles of figures generally, we must go round, producing all the sides in the direction of our progress, assigning the positive or negative sign according as the angle is not or is re-entrant.

Thus in figure 2 the defect is

$$\alpha + \beta - \gamma + \delta + \epsilon - 4\mathrm{R}.$$

Defining defect in this way, it is easy to prove that

The defect of any rectilineal figure is equal to the sum of the defects of any rectilineal figures of which it may be supposed to be composed.

Cor. Hence if one rectilineal figure lie wholly within another the defect of the former is not greater than that of the latter.

Hence follows at once the following important proposition:—

If the defect of any triangle whose sides are finite be zero, then the defect of every finite triangle must be zero.

For if ABC (fig. 3) be a triangle whose defect is zero, then, by applying to its sides three triangles, each congruent with itself, as shown in the figure, we evidently construct a triangle A′B′C′, having the same angles as ABC, and hence zero defect, each of whose sides is double a corresponding side in ABC. We may repeat this process with A′B′C′, and so on. Hence we may construct a triangle, having zero defect, large enough to contain within it any finite triangle whatever. But the defect of any triangle cannot be greater than that of a triangle within which it is contained, and the defect cannot be less than zero; hence the defect of every finite triangle must be zero, if the defect of any one finite triangle be zero.

Thus in *hyperbolic space*, as defined above, we are shut up to one or other of two alternatives. *Either the defect of a triangle is always positive or it is always zero.*

If we take the latter alternative, we get Euclidean or homaloidal space; and, from the defining property by which we have characterised it, we can prove Euclid's parallel axiom, and develop Euclid's geometry in his or any other equivalent manner.

Having separated out homaloidal space, let us now consider more closely hyperbolic space proper, in which the defect is always positive.

The fundamental proposition to be proved is the following.

The defect of a triangle (and consequently the defect of any plane rectilineal figure) is proportional to its area.

Various proofs of this proposition might be given. I select that which depends on the properties of the curves of equidistance from a straight line, because the intermediate propositions are the analogues in hyperbolic space to the propositions regarding parallels and parallelograms that are given in the latter part of Euclid's first book.

If in any plane perpendiculars of constant length be erected upon a given straight line, their extremities generate two curves which I shall call the equidistants, the two equidistants corresponding to a given length of the perpendicular may be called conjugate equidistants.

The equidistant is a self congruent line.

For if we take any piece AB (fig. 4) of the given line, and LM the corresponding piece of the equidistant, and if also A′B′ = AB and L′M′ be corresponding points to A′ and B′, then, if we place A′B′ on AB, L′ and M′ will coincide with L and M, and, if A′P′ = AP, Q′ will coincide with Q, and so on. Hence the piece L′M′ is congruent with the piece LM.

The equidistant is at every point at right angles to the generating perpendicular.

This is at once evident by considering two equal pieces (fig. 5) LP and LQ of the equidistant on either side of L, and the corresponding points A and B on the straight line, so that OA = OB. We have LOAP ≃ LOBQ, hence ∠ OLP = ∠ OLQ, each = R.

The equidistant in hyperbolic space is a curved line, concave towards the given line.

Let LQM (fig. 6) be a piece of the equidistant, LM a straight line cutting the perpendicular through P, the middle point of AB, in R. Then LRPA ≃ MRPB. Hence ∠ PRL = ∠ PRM = R, and the angles at P are each = R, therefore ALR < R.

But ∠ QLA = R, therefore LQ falls above LRQ, however small the distance AB may be; in other words, LQM is concave towards AB.

Every straight line terminated by a pair of conjugate equidistants to a given straight line is bisected by the given straight line, and makes equal alternate angles with the equidistants, &c.

If AB (fig. 7) be the given straight line, XP and YQ the equidistants, POQ the line terminated by the equidistants, then the proposition follows at once by observing that, if AP and BQ be perpendiculars to AB, then AOP ≏ BOQ.

The common perpendicular to two conjugate equidistants is the least distance between them, the oblique distances are greater according as the angle they make with the perpendicular is greater, and the length of an oblique can be increased without limit.

It will be seen that conjugate equidistants are analogous to Euclidean parallels. The analogy may be carried much farther.

If equal arcs of two conjugate equidistants be joined towards the same parts by two straight lines, the figure so formed may be called a *hyperbolic parallelogram.*

A mixed triangle whose base is the arc of an equidistant, whose two remaining sides are straight lines, and whose vertex lies on the conjugate equidistant, may be called a *hyperbolic triangle.* The following propositions are then very easily proved.

The sum of the three angles of a hyperbolic triangle is 2R.

The opposite straight sides of a hyperbolic parallelogram are equal to one another; its diagonals bisect one another in **a** *point on the straight line to which the equidistants that form its curved sides belong; and each diagonal divides it into two congruent hyperbolic triangles.*

A series of propositions analogous to those of Euclid, Book I., 35–41, may be proved very easily; we have only to substitute hyperbolic parallelograms and triangles for ordinary parallelograms and triangles, and conjugate equidistants for parallels. In particular, we see (fig. 8) that

Two hyperbolic triangles CAOB, DAOB, which have for common base the arc AOB of an equidistant (and consequently have their vertices on the conjugate equidistant) are equal in area.

Hence follows at once that—

The rectilinear triangles CAB, DAB on the same chord of an equidistant, whose vertices lie on the conjugate equidistant, are equal in area and defect.

N.B.—the defect is 2 ∠ OAB in both cases. It is obvious that, if we join the middle points of the sides of any triangle, the extremities of its base lie on an equidistant to the line so drawn, and the vertex lies on the conjugate equidistant. Bearing this in mind, the properties of equidistants enable us to establish the following propositions :—

We can always construct an isosceles triangle whose base is equal to one side of a given triangle, and whose area and defect are the same as those of the given triangle.

*Given two triangles, we can always transform one or other of them into another of equal area and defect which has one of its sides equal to one of the sides of the remaining triangle.**

Hence two triangles that have the same area must have the same defect, and conversely, for we can transform them into a pair of isosceles triangles on the same base without altering either area or defect. It is obvious that two such triangles must be congruent if they are equal in area, and hence they must be equal in defect; and from what I have proved concerning the defect of composite figures, the converse follows with equal ease.

Hence *the area of a triangle is proportional to its defect.* Hence, ρ being a certain linear constant, characteristic of a hyperbolic space, and A the area of a rectilineal triangle of defect δ, we have

$$A = \rho^2 \delta .$$

A great variety of very important conclusions can at once be drawn from this formula. I mention some of the most interesting.

Since $\delta = \frac{A}{\rho^2}$, if ρ be infinite, then $\delta = 0$ for every triangle of finite area; in other words, homaloidal space is simply a hyperbolic space whose linear constant is infinite. This conclusion may be looked at from another, but mathematically equivalent, point of view. Let us imagine a hyperbolic space of given linear constant ρ.

* I leave the reader to consider and settle for himself whether a simpler proposition than the above could be established. In particular he should consider the following problem in hyperbolic geometry:—"To construct an isosceles triangle of given area on a given base."

If we take a region in this space whose greatest linear dimension is an infinitely small fraction of ρ, then the defect of every triangle within that region will be infinitely small, and its geometry will not differ sensibly from that of a homaloidal space. This is often expressed by saying that hyperbolic space is homaloidal in its smallest parts.

It appears, therefore, that, even in hyperbolic space, Euclid's planimetry will apply to infinitely small figures. For instance, the ratio of the circumference of a circle to its diameter will be $\pi = 3{\cdot}14159 \ldots$ (the ordinary transcendental constant), when the diameter is made infinitely small. We may, therefore, if we please, measure our angles in radians (circular measure), and in fact use all the formulæ of homaloidal plane trigonometry, if proper restrictions be observed.

It should also be noticed that the existence of this length ρ related to the space, but not *directionally* related, suggests the possibility of explaining the properties of tridimensional space by subsuming it in a space of four or more dimensions. I have not chosen to enter into speculations of this nature, partly because their development has been entirely analytical hitherto; and partly because, so far as I can see at present, it may be justly contended that the conceivability of hyperspace of three dimensions rests on different grounds from that which we must necessarily assume when we attempt to add another dimension. In this, however, I may be but one of those whom Gauss playfully called Bœotians.*

* Before leaving this part of the subject, I may mention the curious solution of the problem of dividing a plane in hyperbolic space into a network of regular polygons.

If n be the number of sides of each polygon, p the number of polygons round a point of the network, A the area of each of the n-gons, then

$$A = n\pi\rho^2\left(1 - \frac{2}{n} - \frac{2}{p}\right),$$

with the condition $\frac{1}{n} + \frac{1}{p} < \frac{1}{2}$.

Suppose, for instance, we wish to divide a plane into squares, *i.e.*, regular four-sided figures. Then $n = 4$. If $p = 4$, *i.e.*, if the angles of the square be right angles, $A = 0$, which does not, strictly speaking, give a solution. The next case is $p = 5$, so that $A = \frac{2}{5}\pi\rho^2$ is the area of the smallest finite square with which we could pave a plane floor. Of course there are an infinite num-

Theory of Parallels.

If O (figs. 9 and 10) be any point outside a line, P any point in it to the right of the foot of the perpendicular, then the limiting position of OP, when P is moved in the direction DI to the right, without limit, is called the parallel through O to DI. The corresponding limiting line on the other side of OD is called the parallel through O to DI′.

Thus

$$\begin{aligned} OK &// DI \\ OK' &// DI' . \end{aligned}$$

It is obvious, from the uniformity of space, that OK and OK′ make equal angles with OD. Whether they are parts of the same line or not, remains to be seen.

As P moves off along DI the angle at P diminishes without limit.

This is easily shown (fig. 10) by taking $PP_1 = OP$, $P_1P_2 = OP_1$ and so on *ad. inf.*

In homaloidal space the parallel to DI through O is the perpendicular to DO at the point O: for the sum of the three angles of the triangle DPO is always 2R, and P diminishes without limit, hence the angle at O approaches nearer to R than by any assignable quantity.

Thus in homaloidal space the two parallels OK, OK′ are parts of the same straight line, and all the lines through O cut IDI′, except the parallel, which may be said to cut it at an infinite distance. In the language of modern geometry there is but one point at infinity on the line IDI′.

In hyperbolic space there are two parallels through a given point to a given straight line.

For as we move P away from D the area of ODP, and consequently its defect, constantly increases, but the angle OPD constantly diminishes, hence the angle at O can never exceed a certain angle which is less than a right angle.

It follows, therefore, that if we take any line IDI′ and any external point O, we must classify the lines through O as follows :—(1) intersectors, (2) non-intersectors, (3) two parallels.

ber of solutions, the angles of the squares becoming less and their area greater as p increases. The area of the greatest possible square tile that we could use would be $2\pi\rho^2$, but the lengths of the sides would be infinite.

In figure 11 KOL′ and K′OL are the two parallels; all lines lying in the angles KOL, K′OL′, are non-intersectors, all those lying in KOK′, LOL′ are intersectors. The fact that in hyperbolic space there are two parallels through a given point to a given straight line is expressed in modern geometry by saying that in hyperbolic space a straight line has two distinct real points at infinity.

After what has been laid down, the following propositions either are immediately evident, or can be proved with very little trouble.

If a line is parallel to another at any point, it is so at every point of itself.

Parallelism is mutual.

Lines which are parallel to the same line are parallel to one another.

Lines that are parallel continually approach one another on the side towards which they are parallel.

Non-intersectors in the same plane have a minimum distance, which is the common perpendicular.

The angle which a parallel through O to L makes with the perpendicular on L is called the parallel angle.

The parallel angle is a function of the length of the perpendicular, increasing when the perpendicular diminishes.

If θ be the angle, p the length of the perpendicular, then it may be shown by methods which I shall presently explain that

$$\tan \tfrac{1}{2}\theta = e^{-\frac{p}{\rho}},$$

$$\text{When } p=0,\ \theta=\frac{\pi}{2};\ \text{when } p=\infty,\ \theta=0.$$

Geometry of Elliptic Space.

For simplicity I take single elliptic space, but there will be no difficulty in modifying what follows so as to make it apply to double elliptic space.

In single elliptic space every straight line returns into itself; and two straight lines intersect in only one point. Thus, starting from any point P, and proceeding in any direction continuously, we at last return to the point P; the length L travelled over in this process is called the length of the *complete straight line.*

It is obvious that in single (as well as in double) elliptic space

two intersecting complete straight lines enclose a plane figure. Such a figure I call a *biangle.*

Two biangles are congruent when their angles are equal. All complete straight lines are of the same length, and all the straight lines emanating from the same point intersect in the same second point.

These propositions are all equivalent to one another, and are equally true for single or double elliptic space. The last of them is a mere truism for *single* elliptic space. The following demonstration, which holds good for single or double elliptic space, may help to render the matter clearer.

Let APBQA A′P′B′Q′A′ (fig. 12) be two biangles hav ng the angles A and A′ equal. If A′B′ be placed on AB so that A lies on A′, and A′P′ along AP, then A′Q′ will lie along AQ, since the angles at A are equal; hence by the fundamental property of a straight line APB and A′P′B′ must wholly coincide, and AQB and A′Q′B′ must wholly coincide; and hence B′ must fall on B. It is to be noticed that the biangles are multiply congruent.

Next, suppose AKA′, AK′A′ (fig. 13) to be any pair of intersecting straight lines. Let AL bisect the angle A and cut the lines in J and J′. Since AJ and AJ′ are equiangular biangles, they are congruent; from this it follows at once that J and J′ must coincide with each other, and therefore each with A′. Hence the bisector of the angle A passes through A′; and it and AKA′ and AK′A′ are all of equal length. We may next bisect either of the halves of A, and so on; and we may double any of the angles thus obtained as often as we please. Hence the propositions stated above are completely proved. The length L of a complete straight line is therefore an absolute linear constant which characterises an elliptic space.

In single elliptic space the least distance between two points can never be greater than $\frac{1}{2}L$, and the greatest distance can never be greater than L.

This is obvious, since the whole length of a complete straight line through the two points is L.

If we consider the plane determined by two intersecting straight lines AOA, BOB, and if we pass from O along OA through a length L, we return to O, but find ourselves on the opposite side of the plane to that from which we started, and only arrive at the same point O

on the same side as before by travelling once more through a length L.

This curious conclusion is an immediate result of the fact that straight lines are re-entrant and intersect only once. (In double elliptical space the apparent anomaly does not occur on account of the double intersection.)

The best way of representing the thing to the mind that I can think of is to imagine a rigid body composed of three rectangular arrows Ix, Iy, Iz (fig. 14). Ix slides along OA; Iy passes through a ring which slides on OB (being long enough never to slip out); Iz is, of course, determined in position when Ix and Iy are fixed in any positions.

In starting from O, let Ix and Iy be horizontal and Iz vertical; then slide Ix along OA. Ix will at last return along A′O. The ring will return along B′O. It is obvious, therefore, that, at our first return to O, Iz must be downwards, for, since the system of arrows is rigid, one who plants himself with feet at I, head at z and looks along Ix must see y to his left as he did at starting.

It is obvious that during the journey Iy as well as Iz has rotated through 180°, a repetition of the process rotates both through 180° more, and then everything is as before.

If we cause a complete straight line of length L to revolve through 360°, always remaining perpendicular to a given line, it will sweep out the two sides of a *complete plane.*

It follows at once, therefore, that the area of a complete plane, taking into account both sides, is finite, and the same for every complete plane. This I shall call P in the meantime. We also see, in accordance with what was proved before, that the two sides of the complete plane are not distinct, since we can pass continuously upon the plane from a point on one side to the same point on the other side.

Those who find difficulty in realising this property of the plane in single elliptic space should take a ribbon of paper, twist it through 180°, and then gum the ends together. A surface is thus formed which has the property that one can trace a continuous line upon it from a point on one side to a point exactly opposite on the other side.

After what has been laid down the following propositions are obvious.

They are given by Newcomb in an extremely interesting article to which reference was made above. I arrange them in the order which best suits what has gone before.

All the perpendiculars in a given plane to a given straight line intersect in a single point, whose distance from the straight line is $\frac{1}{2}L$.

Conversely, the locus of all the points at a distance $\frac{1}{2}L$ on straight lines passing through a given point in a given plane is a straight line perpendicular to all the radiating lines.

The fixed point is called the *pole*, and the straight locus its *polar*.

If we cause the given plane to rotate about the polar the pole describes a straight line which may be called the conjugate of the given polar.

The relation of these two lines is mutual, every point on one being at a distance $\frac{1}{2}L$ from every point on the other.

Without dwelling farther upon propositions of this kind, I proceed at once to establish the fundamental proposition concerning the sum of the angles of a plane triangle. I might follow a course like that adopted for hyperbolic space, but a much simpler method suggests itself at once as applicable to finite space.

In the first place, since a complete plane is generated by the revolution of a complete straight line through 360°, it follows that the area of a biangle whose angle is A° is $\frac{A}{360}$ P.

In figure 15 let ABC be any triangle. Produce the sides to form biangles. Each of the biangles departs from the vertex on the upper side of the plane and returns to the vertex on the lower side. To make this clear areas in the neighbourhood of ABC in the figure are shaded with vertical lines when reckoned on the upper and with horizontal lines when reckoned on the lower side of the plane. A glance will show that if we take the three biangles they overlap the triangle ABC thrice, and that the rest of the plane is covered every where once on one side or the other, but nowhere on both sides. Hence, Δ denoting the area of the triangle, we have

$$\frac{A}{360}P + \frac{B}{360}P + \frac{C}{360}P = \tfrac{1}{2}P + 2\Delta$$

$$\Delta = \frac{A+B+C-180}{360}P.$$

If, therefore, we define $A° + B° + C° - 180°$ as the *excess* of the triangle, we have the proposition that—

The excess of every triangle is positive, and is proportional to its area.

The conclusions drawn above (p. 650) for hyperbolic space follow here, *mutatis mutandis.* In particular, we see that we may apply Euclidean planimetry to infinitely small figures. On this remark we can, as will be done later, found a system of planimetry for elliptic space, and determine P. The result is $P = \frac{4L^2}{\pi}$. Hence, writing ρ for $\frac{L}{\pi}$, and ϵ for the radian measure of the excess, we have

$$\Delta = \rho^2 \epsilon$$

where ρ is a linear constant characteristic of the elliptic space.

It is easy after what has now been established to work out the propositions corresponding to Euclid's first book. The conclusions will, of course, be subject to certain modifications, but these are easily found. I may mention in particular that the propositions concerning the curves of equidistance already given for hyperbolic space, hold with very slight modification for elliptic space, the main difference being that the equidistants are convex instead of concave to the given straight line.

Theory of Parallels.

In elliptic space there is, of course, no such thing as a parallel, because there are no infinitely distant points on a straight line.*

If O (fig. 16) be a point outside the line IDI′; then it is easy to see that the two segments of the perpendicular from O are respectively the least and greatest distances from the given line. If OD be the least distance, then, as OP, starting from OD, revolves about O, OP continually increases, until it has rotated through 180°, and then it is at its maximum, after which it decreases again.

It can easily be shown that, as OP revolves from OD, the angle OPD decreases, until OP is perpendicular to OD, and then OPD is at its minimum value. After that, as may be easily shown by producing the line backwards through O, the angle again increases.

* In the language of modern geometry the points at infinity on a straight line in elliptic space are imaginary.

The line OI, perpendicular to OD, is all that there is in elliptic space to represent a parallel through O to the line I′DI.

General Conclusions.

If I have succeeded in my attempt to explain the results of modern research concerning the axioms of geometry, it will be apparent that, even if we overlook the possibility of space being non-uniform, in the sense of having properties depending on position and direction, it is still possible to develop three self-consistent kinds of geometry—the hyperbolic, the homaloidal, and the elliptic. It is impossible, it appears to me, to say on *à priori* grounds that any one of these is more reasonable than the others. If, therefore, *à priori* ground is to be sought for the axioms of geometry, such tests of its firmness "as the inconceivability of the opposite" and others like it are not to be relied upon. They are merely an appeal to ignorance.

If, on the other hand, we view the question from the side of experience, three alternatives are open to us. We may hold that space is homaloidal and therefore infinite. In this case we extend to the infinite part of space which we do not know the results of our experience of the finite part of it that we do know.

Again, we may hold that space is hyperbolic and therefore infinite. In this case experience teaches us that the radius of the sphere of our experience is infinitely small compared with the linear constant of space; for Lobatschewsky calculated from astronomical observations the sum of the three angles of triangles whose smallest sides were about double the distance of the earth from the sun, and found that the difference from two right angles was not greater than the probable error of observation.

Lastly, we may suppose that space is elliptic and therefore finite, in this case we must admit that our experience extends to but an infinitely small fraction of its whole extent, since no sensible excess can be found in the largest triangles with which we are acquainted.

Before leaving this subject, it may be well to illustrate with some care what is meant by the words finite and infinite as I have used them. They have, of course, a purely relative meaning. In the geometry of homaloidal space no distinction can be built on the relative dimensions of figures apart from their form. Owing to the

existence of similar figures, the geometrical experience of a cheese mite in homaloidal space would not be different from that of a being one of whose habitual walking steps was from the sun to the dog star.

In hyperbolic or elliptic space the case is otherwise. In either of these two kinds of space we might divide intelligent beings into two classes according to their bodily dimensions. We might have a race of micranthropes, whose bodily dimensions and the radius of whose sphere of experience were infinitely small compared with the linear constant of space. For instance, if the space were elliptic, the world of the micranthropes would be but an infinitely small fraction of the elliptic universe. It must be noticed, however, that from the point of view of a micranthrope, his world need not be a prison-house by any means, for he would compare it not with the linear constant of universal space, of whose magnitude he must necessarily be ignorant, but with some arbitrary standard such as the length of his own arm, and so considered his world would to him be infinite, if we only suppose him small enough. Again, we might have a race of macranthropes, whose bodily dimensions were comparable with the linear constant of space. In the case of an elliptic and finite space, we could, of course, conceive one of these himself so great that there would not be room enough in the universe for another as great.

The geometry of the micranthropes would, of course, be homaloidal. The axioms of Euclid would appear to them strictly in accordance with experience, and, although they lived in part of an elliptic or hyperbolic space, their prejudices would render the conceptions of the general properties of such a space as difficult to them as they are to us. On the other hand, the geometry of the macranthropes would be elliptic or hyperbolic, as the case might be. A hyperbolic macranthrope would, of course, be familiar with the fact that the defect of a triangle diminishes as its area diminishes. If he were a mathematician he would be aware of the relation of proportionality, and might speculate concerning triangles of zero defect, much as we do about absolute zero of temperature. If Euclid's geometry were to fall into the hands of an instructed macranthrope, he would very likely regard it as the production of some macranthropic lunatic, who had meditated on the fact that the defect of a triangle diminishes with its area, until he had so far lost his wits as to commit the ὑστερον προτερον of discussing the construction of an equilateral triangle

before proving that when two straight lines cut one another the vertically opposite angles are equal!

Appendix on the Trigonometry of Elliptic and Hyperbolic Space.

The following appears to me to be the simplest, and at the same time the most instructive way of establishing the Trigonometry of Elliptic and Hyperbolic Space.

The method might, indeed, by assuming proper axioms, be made to take the place of the preceding synthesis. As it is, I shall base it upon the results of that synthesis. What I shall want are mainly the propositions concerning the excess or defect of plane triangles, and the conclusion founded on them that homaloidal trigonometry may be applied to figures, all of whose dimensions are infinitely small compared with the linear constant of space.

Let KA and LB (fig. 17) be two straight lines in the same plane at an infinitely small distance apart. They may be either non-intersectors, whose minimum distance d is infinitely small, or intersectors which make a very small angle α with each other at their point of intersection.

Let KL, AB, CD be lines making equal angles with KA and LB; and let $KA=LB=r$, $AC=BD=dr$, $AB=D$, $CD=D+dD$, where dr is infinitely small compared with r, dD infinitely small compared with D; D of course is infinitely small compared with ρ, the linear constant of space.

Further, let $\angle LBA = \angle KAB = \frac{\pi}{2} - \theta$, and $\angle LDC = \angle KCD = \frac{\pi}{2} - \theta - d\theta$.

Since all the dimensions of ABDC are infinitely small compared with ρ, we may apply Euclidean trigonometry. Draw Bm parallel to AC. Then $\angle ABm = \frac{\pi}{2} - \theta$, $AB = Cm$, and $Dm = dD$.

$$2 \sin \tfrac{1}{2} DBm = 2 \sin(-\theta) = \frac{dD}{dr};$$

$$\theta = -\tfrac{1}{2} \frac{dD}{dr}. \qquad . \quad . \quad . \quad . \quad . \quad . \qquad (1)$$

Now the excess of ABDC $= 2\left(\frac{\pi}{2} + \theta\right) + 2\left(\frac{\pi}{2} - \theta - d\theta\right) - 2\pi = -2d\theta$; and its area $= Ddr$. Hence

$$Ddr = \rho^2\epsilon = -2\rho^2 d\theta.$$

which gives

$$2\frac{d\theta}{dr} + \frac{D}{\rho^2} = 0.$$

Whence by (1)

$$\frac{d^2D}{dr^2} + \frac{D}{\rho^2} = 0. \qquad . \quad . \quad . \quad . \quad . \qquad (2)$$

This is the equation for Elliptic Space ; that for Hyperbolic Space is of course

$$\frac{d^2\mathrm{D}}{dr^2} - \frac{\mathrm{D}}{\rho^2} = 0. \qquad (2')^*$$

From the equation (2) we get at once

$$\mathrm{D} = \rho\alpha \sin\frac{r}{\rho}, \qquad (3)$$

r being measured from the intersection of the lines, and the constants of integration determined by the condition

$$\mathrm{D} = \alpha r$$

when r is infinitely small compared with ρ, which of course includes the condition $\mathrm{D} = 0$ when $r = 0$.

The corresponding formulæ for Hyperbolic Space are

$$\mathrm{D} = \rho\alpha\left(\frac{e^{\frac{r}{\rho}} - e^{-\frac{r}{\rho}}}{2}\right)$$

$$= \rho\alpha \sin h\frac{r}{\rho} \qquad (4)$$

for a pair of intersectors ; and

$$\mathrm{D} = d\left(\frac{e^{\frac{r}{\rho}} + e^{-\frac{r}{\rho}}}{2}\right)$$

$$= d \cos h\frac{r}{\rho} \qquad (5)$$

for a pair of non-intersectors, r being measured in the one case from the intersection, in the other from the points of minimum distance.

From the formulæ (3), (4), and (5) all the trigonometry of Elliptic and Hyperbolic Space can be deduced most readily. I append one or two applications, and select for my purpose important formulæ, but anything like a complete development would be out of place here. †

* The differential equations (2) and (2′) contain all the metrical properties of elliptic and hyperbolic space. (2) suggests that a pair of straight lines diverging at a small angle from a point might intersect again in distinct points any number of times. The proposition proved above for elliptic space generally, that all the lines radiating from any point intersect in the same second point, seems, however, to compel us to conclude that at the point where any line intersects another for the second time, it must return into itself; for a line can be brought by continuous rotation into coincidence with its prolongation, hence we must reach the same second point of intersection in whichever direction we proceed from the first point. I can see no way out of this at present; and if there is none, it would appear that we cannot get beyond double elliptic space, even if we can consistently get so far.

† I may refer the reader to Frischauf, "Elemente der Absolute Geometrie," Leipzig, 1876 ; Lobatschewsky, Crelle, xvii. p. 295 ; Klein, Annalen der Mathematik, iv. p. 573, vi. p. 112, &c. ; Cayley, Annalen der Mathematik, v. p. 630.

Area of Complete Plane and Total Volume of Elliptic Space.

The area of a biangle having the infinitely small angle α is

$$\rho\alpha\int_0^{\pi\rho} \sin\frac{r}{\rho}\,dr = 2\rho^2\alpha\,.$$

Hence
$$P = 4\pi\rho^2 = \frac{4L^2}{\pi} \qquad (6)$$

From this result we can deduce very easily the total volume S of elliptical space (single). The locus of the most distant points on the radii through any point of space is a plane. Suppose this plane divided up into infinitely small regular quadrilaterals (squares) of side k. The volume dS contained by four radii drawn to the vertices of one of these figures is

$$dS = k^2\int_{-\frac{\pi}{2}}^{\frac{\pi}{2}\rho} \sin\frac{2r}{\rho}\,dr = \frac{\pi}{2}k^2\rho\,;$$

Hence
$$S = \frac{P}{2k^2}dS = \frac{P\rho}{4}\,,$$
$$= \pi^2\rho^3\,,$$
$$= \frac{L^3}{\pi} \qquad (7)$$

This curious result can also be obtained by calculating the volume swept out by a complete plane rotating through 180° about any line in it.

Formulæ for Right-Angled Triangles.

Let ACB (fig. 18) be a triangle right angled at C. Let $BAb = dA$, $Bb = da$. $CbA = B + dB$. If Bm be perpendicular to Ab, then $bm = dc$.

We have at once by (3)

$$\sin B da = \rho \sin\frac{c}{\rho}dA \qquad (8)$$

Also
$$dc = da\cos B \qquad (9)$$

Calculating the area BAb in the two different ways we get

$$\int_0^c Ddc = \rho^2\epsilon\,.$$

Whence
$$dA\rho^2\left(1 - \cos\frac{c}{\rho}\right) = \rho^2(dA + dB)$$

i.e.,
$$dB = -\cos\frac{c}{\rho}\,dA \qquad (10)$$

Whence $$dc \tan B = -\rho \tan\frac{c}{\rho}\, dB \quad . \quad . \quad . \quad . \quad (11)$$

From these equations we get successively:

$$\sin B \sin\frac{c}{\rho} = \sin\frac{b}{\rho} \quad . \quad . \quad . \quad . \quad \text{I.}$$

$$\cos\frac{a}{\rho} \cos\frac{b}{\rho} = \cos\frac{c}{\rho} \quad . \quad . \quad . \quad . \quad \text{II.}$$

$$\sin A \cos\frac{b}{\rho} = \cos B \quad . \quad . \quad . \quad . \quad \text{III.}$$

$$\tan\frac{b}{\rho} \cot\frac{c}{\rho} = \cos A \quad . \quad . \quad . \quad . \quad \text{IV.}$$

For hyperbolic space we get in like manner

$$\sin B \sin h\frac{c}{\rho} = \sin h\frac{b}{\rho} \quad . \quad . \quad . \quad . \quad \text{I}'.$$

$$\cos h\frac{a}{\rho} \cos h\frac{b}{\rho} = \cos h\frac{c}{\rho} \quad . \quad . \quad . \quad . \quad \text{II}'.$$

$$\sin A \cos h\frac{b}{\rho} = \cos B \quad . \quad . \quad . \quad . \quad \text{III}'.$$

$$\tan h\frac{b}{\rho} \cot h\frac{c}{\rho} = \cos A \quad . \quad . \quad . \quad . \quad \text{IV}'.$$

The reader will observe that these are simply the formulæ included in Napier's rules for right-angled spherical triangles. The only modification being that in hyperbolic space hyperbolic functions take the place of circular functions. In other words, the trigonometry of single elliptic space is identical with the geodetic trigonometry of a sphere, although it would not be correct to say that the planimetry of single elliptic space is identical with the geodesy of a sphere.

For hyperbolic space the analogue is the pseudo-spherical surface of Beltrami.

Parallels.

As an illustration of the application of the above formulæ to parallels, I shall find the parallel angle in hyperbolic space.

Taking formula IV′, if we make B move off to an infinite distance, then AB becomes the parallel to CB. A is then the parallel angle corresponding to b. Now since $c = \infty$ we have

$$\cot h\frac{c}{\rho} = 1,$$

therefore

$$\cos A = \tan h\frac{b}{\rho} = \frac{e^{\frac{b}{\rho}} - e^{-\frac{b}{\rho}}}{e^{\frac{b}{\rho}} + e^{-\frac{b}{\rho}}} \quad . \quad . \quad . \quad . \quad . \quad (12)$$

Whence $$\tan\frac{A}{2}=e^{-\frac{b}{\rho}},$$

the relation stated above (p. 653).

Non-Intersectors.

As an example of the trigonometry of non-intersectors, I select the following formulæ, the proof of which I leave to the reader.

If KA and LB be two non-intersectors, K and L the points of least distance, $KA=LB=r$, $KAB=LBA=\phi$, $KL=d$, $AB=D$.

Then $$\sin h\frac{D}{2\rho}=\sin h\frac{d}{2\rho}\cos h\frac{r}{\rho} \quad . \quad . \quad . \quad . \quad (13)$$

$$\sin\phi=\frac{\cos h\dfrac{d}{2\rho}}{\cos h\dfrac{D}{2\rho}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

The results of (6) to (11) are given by Newcomb (Borchardt, lxxxiii. p. 293) mostly without demonstration. He assumes formula (3) as one of the axioms on which he bases his synthesis. Although I have read most of the original literature on the subject, I am more immediately indebted to Newcomb and Frischauf for the materials of the foregoing sketch.

2. Note on the Theory of the "15 Puzzle." By Professor Tait.

[After this note had been laid before the Council, the new number (vol. ii. No. 4) of the "American Journal of Mathematics" reached us. In it there are exhaustive papers by Messrs Johnston and Story on the subject of this American invention. The principles they give differ only in form of statement from those at which I had independently arrived. I have, therefore, cut down my paper to the smallest dimensions consistent with intelligibility.—P. G. T.]

The essential feature of this puzzle is that the circulation of the pieces is necessarily in rectangular channels. Whether these form four-sided figures, or have any greater (*even*) number of sides, the number of squares in the channel itself is always even. (This is the same thing as saying that a rook's re-entrant path always contains an even number of squares. This follows immediately from the fact that a rook always passes through black and white squares alternately. The same thing is true of a bishop's re-entering

path, for it is a rook's upon a new chess-board formed by the alternate diagonals of the squares on the original board.) That there may be circulation in the channel, one of its squares must be the blank one.

Hence an *odd* number of pieces lies along the channel, and, therefore, when they are anyhow displaced along it, so that the blank square finally remains unchanged, the number of interchanges is essentially *even*.

Thus to test whether any given arrangement can be solved, all we need know is how many interchanges of two pieces will reduce it to the normal one. If this number be even, the solution is possible. To find the number of interchanges, we have only to write in pairs the numbers occupying the same square in each arrangement, and divide them into groups, such as $\begin{matrix} a & b & c & d \\ b & c & d & a \end{matrix}$, which form closed cycles. Here there are *four* pairs in the group, which correspond to *three* interchanges, because $\begin{matrix} a & b \\ b & a \end{matrix}$ is one interchange.

Dr Crum Brown suggests the term *Aryan* for the normal arrangement, with the corresponding term *Semitic* for its perversion. Similarly *Chinese* would signify the Aryan rotated right-handedly through a quadrant, and *Mongol* Semitic rotated left-handedly through a quadrant.

Now it is easily seen that Aryan is changed into Semitic, and Chinese into Mongol, or *vice versâ*, by an odd number of interchanges. Similarly Aryan and Mongol, and Semitic and Chinese, differ by an even number of interchanges.

Hence any given arrangement must be either Aryan or Semitic. The former can be changed into Mongol, the latter into Chinese.

Unless the 6 and 9 be carefully distinguished from one another every case is solvable, for if it be Semitic the mere turning these figures upside down effects one interchange and makes it Aryan.

The principle above stated is, of course, easily applicable to the conceivable, but scarcely realisable, case of a rectangular arrangement of equal cubes with one vacant space.

3. On the Constitution of Adult Bone-Matrix and the Functions of Osteoblasts. By De Burgh Birch, M.B., Demonstrator of Physiology in the University of Edinburgh.

(Abstract.)

By tearing thin sheets from the surface of decalcified bones, Sharpey first demonstrated the lamellar nature of bone, and also that these lamellæ have a fibrous structure.

The fibres, consisting of two sets crossing each other, were thought to be interwoven in each lamella, and contiguous fibrous laminæ to be separated from each other by a homogeneous ground substance in which the lime salts were mainly situated; alternations of fibrous and homogeneous layers giving rise to the appearance of lamellation.

From the researches of V. Ebner and my own investigations, the matrix is undoubtedly throughout fibrous, the lamellar appearance being due to an alternation of layers in which the directions of the fibres differ.

By digesting with trypsin sections of decalcified bone, the white fibrous tissue elements of which had been rendered indigestible by protracted treatment with chromic acid, the interfibrillar substance was entirely removed.

In sections thus treated, an alternating band and dot series obtained, where one set of lamellæ was cut longitudinally and the other transversely.

In Haversian systems the entire system in section presented the same appearance, or the lamellation was perceptible only over certain parts, this difference being due to the fact that in some cases the fibres are arranged spirally, whilst in others they run parallel and transversely to the long axis.

Trypsin digestion further caused the isolation of lacunar membranes with their tubular prolongations the canaliculi; these were found united to similar membranes lining the interior of Haversian canals.

Such membranes were also met with in the substance of the

Haversian system, indicating the probable occurrence of periods of cessation, in the deposition of the systemic lamellæ, of some duration.

The situation of the lacunæ was found to be almost universally in the thickness of a lamella.

The nature of the lacunar membrane is not elastic.

Functions of Osteoblasts.—For the ordinary classification of bone formation, it seems to me that the following is a subdivision more in accordance with observed facts. Bone is produced under conditions where

1. There is preformed fibrous tissue,
2. Where no preformed tissue occurs.

Subperiosteal bone formation comes under the first, and under the second, the production of bone upon cartilage spicules and in the excavations for Haversian canals.

There are situations where bone grows at the expense of tendons inserted into them; the level of ossification is indicated by a regular and rapid proliferation of the connective tissue corpuscles, the resulting corpuscles becoming lacunar cells and secreting the calcareous matrix, the fibrous tissue of the tendon becoming that of the osseous matrix.

Subperiosteally the fibrous tissue of the osseous matrix is preformed as the reticulum of the periosteum. This reticulum is produced by connective tissue corpuscles of the periosteum; other protoplasts secrete the interfibrillar calcareous cement.

From these appearances and others observable upon the spicules of cartilage in subepiphysal bone production, it seems justifiable to ascribe the production of the fibrillar elements on the one hand, and the interfibrillar calcareous cement with lacunar and canalicular membranes on the other to two sets of protoplasts, which I would designate as "spinners and plasterers."

4. On two unrecorded Eggs of the Great Auk (*Alca impennis*) discovered in an Edinburgh Collection; with remarks on the former existence of the bird in Newfoundland. By Robert Gray. [Specimens exhibited.]

The two eggs of the Great Auk which I now exhibit were bought in Dowell's Auction Rooms rather more than a month ago, and formed part of a collection of "birds' eggs, shells, and other natural history specimens" which was disposed of among a lot of miscellaneous property belonging to a legal gentleman of this city. This small collection of eggs had been in the possession of the owner for about thirty years, and the two eggs in question had been purchased by his father from another collector in Edinburgh—a Mr Little—in whose possession it is thought, from collected and trustworthy evidence, the specimens had been at least other thirty years. These eggs, therefore, have probably not changed hands more than once during a period of fifty or sixty years. The present owner of the specimens, Mr Small, animal preserver, George Street, purchased the lot at the sale for £1, 12s., and has since taken great pains to establish the few facts I have stated regarding their history.*

The chief interest in the eggs of the Great Auk arises from the circumstance of the bird itself being now regarded as an extinct species. In the early part of the present century it was reckoned a very rare bird, although it appears to have lingered in Scotland, where it was confined to the Orkney Islands† and St Kilda,‡ until 1821; in Ireland§ until 1834; in Newfoundland,‖ which country may be regarded as having been at one time the stronghold of the species, until probably 1837; and in Iceland¶ until 1844, since

* The two eggs have since been sold by auction in London; one specimen realising £100, and the other £107, 2s. Both are now in the collection of Lord Lilford.

† Montagu, "Ornithological Dictionary," Appendix to Supplement, 1813.

‡ Fleming, "Edinburgh Philosophical Journal," vol. x. 1824.

§ Thomson, "Birds of Ireland," vol. iii. p. 238.

‖ Audubon, "Orn. Biog." 1838.

¶ Newton, "Ibis," 1861, pp. 390–392.

which year all traces of the living bird have been lost. Very great interest has consequently been felt by all classes of naturalists in everything that relates to the Great Auk; and the discovery of two specimens of its egg, in addition to those already recorded as existing in collections, is an event which is sure to create some degree of excitement, not only among egg collectors, but among scientific ornithologists in all parts of the world.

Some years ago, in writing a history of this remarkable bird as a Scottish species, I made a very careful summary of the records of its occurrence in North Britain, from the earliest published accounts of the bird until its final disappearance; and to this account, which is given in the "Birds of the West of Scotland,"* I may perhaps be permitted to refer.†

* Gray, "Birds of the West of Scotland, including the Outer Hebrides," 1871, pp. 441–453.

† I have the pleasure of inserting here the following letter which has been addressed to me since this paper was read. It narrates the capture of the last of the Auks in Scotland :—

"EDINBURGH, 17*th June* 1880.

"MY DEAR SIR,—I think you will be interested in knowing that when at St Kilda on the 14th of this month I found there was a man still living there who assisted at the capture of Fleming's Great Auk in 1821–22.

"Having shown a drawing of an Auk to the collected natives to see if they had any knowledge of it, they said they knew it used to be there long ago, but they had never seen it. Subsequently they told me the man was still there who *caught the last Great Auk.*

"I had him immediately brought to me. His name is Donald M'Queen, Sen. He is a very little man, and is also so much bent, that he does not now stand much higher than the Great Auk did. He said he was 73 years of age, but to all appearance he is considerably more.

"Donald disclaimed having been (as his neighbour reported) the person who actually caught the Auk. He informed me that he was one of four persons in a boat on the east side of the Island when they discovered the bird sitting on a low ledge of the cliff.

"Two of their number (then young men) were landed, one on either side of the bird, and at some distance from it. These two cautiously approached it, whilst he and another boy rowed the boat straight towards the Auk, which ultimately leaped down towards the sea, when one of the youths, having got directly under it, caught it in his arms. The old man with much animation went through the pantomime of grasping a supposed bird in his arms and holding it tightly to his breast.

"A partial error of the old St Kildian served to identify this Auk with Fleming's. He said the people who got it 'tied a string to its leg and killed it.'

Regarding its existence in other parts of the world there can be no reasonable doubt that the Great Auk had, as already mentioned, its headquarters on the rocky and almost inaccessible islets off the coast of Newfoundland. In these localities, if we may judge from the writings of the early navigators, it existed in very large communities, especially during the breeding season, and was taken in boatloads for sustenance to the ships' crews. We read, for example, in Captaine Richard Whitbourne's "Discourse and Discovery of Newfoundland," published in 1622, that "these Penguins" (the name by which the Great Auk was known on the coasts) "are as bigge as geese and fly not, for they have but a little short wing, and they multiply so infinitely upon a certaine island that men drive them from thence upon a boord into their boats by hundreds at a time, as if God had made the innocency of so poor a creature to become such an admirable instrument for the sustentation of man." Other writers speak of parties landing upon the rocks and knocking down with clubs hundreds of Auks, which they carried to their ships to be used as food. In short, we know that nearly all the French sailors of that and later periods trading between Havre de Grace or other French ports and Newfoundland, victualled their ships regularly with Great Auks taken in such places as the Funk or Penguin Islands, which are situated on the east coast; and that on leaving home they regulated their supply of provisions accordingly. It is stated in a letter written from Bristow to Mr Richard Hakluyt of the Middle Temple by Mr Anthonie Parkhurst, and dated the 15th of November 1578, that "the Frenchmen that fish neere the grand baie doe bring small store of flesh with them, but victuall themselves always with these birdes"; * and again, in "a report of the voyage and successe thereof, attempted in the yeere of our Lord 1583 by Sir Humfrey Gilbert, knight, &c., by Mr Edward Haies, gentleman, &c., it is stated that the French men barrell them vp with salt." † Such a fate for the poor Auks was foreshadowed in 1536, in which year Penguin Island, one of their haunts,

"When I told him they did not kill it, he said he might have forgotten what he had heard about it after it was taken away.

"Mr Mackenzie, the factor of the island, who was present, said Donald M'Queen was a trustworthy person, and that I might rely upon his telling the truth.—Yours faithfully, R. Scot Skirving."

* Hakluyt, vol. iii. pp. 172, 173.

† *Ibid.* vol. iii. p. 191.

was visited by a Mr Hore "and divers other gentlemen," who recorded having seen a "great number of foules" which were "very good and nourishing meat"—a hint which appears not to have been lost sight of by subsequent navigators. "A person of the name of Hore, says Forster [Robert Hore], set sail in 1536 from London with two ships—the 'Trinity' and the 'Minion'—about the latter end of April. They arrived at Cape Briton, and from thence went to the north-eastward till they came to Penguin Island, an island situated on the southern coast of Newfoundland, and which was named thus after a kind of sea-fowl which the Spaniards and Portuguese called *Penguins* on account of their being so very fat, and which used to build their nests and to live in astonishing quantities on this little rock."* Four years later (1540) Jacques Cartier in his "Third Voyage" refers to the slaughter of these birds by himself and his crews, and speaks of loading his two vessels with dead Penguins in less than half an hour, as he might have done with stones, so that, not reckoning those that were eaten fresh he had in each vessel four or five tons of them put in salt.

No species of bird, especially one to which the power of flight had been denied, could long survive such wholesale destruction as these French sailors narrate; and as we have already seen that subsequent traders continued to make very serious inroads upon the haunts of the doomed Penguin, it need excite no surprise that by the time the attention of scientific writers was drawn to the bird, it had almost become a rare species. In a published form there is but little to narrate, between the time of Whitbourne's visit and the inquiries made by Audubon when preparing his work on the "Birds of America," in 1831. Writing in 1684, William Dampier † states that he had seen Penguins plentifully on the coast of Newfoundland; and in 1750 George Edwards, author of a meritorious work on the "Natural History of Birds," figured a Great Auk in vol. iii. pl. xlvii., which he states he procured from the master of a Newfoundland fishing-vessel who captured it with fish bait on the fishing banks about a hundred leagues off shore.

* History of the Voyages and Discoveries made in the North, by John Reinhold Forster, J.U.D., London, 1786, p. 290.

† New Voyage round the World, 3d ed., London, 1698.

Again, we find from a "Journal of Transactions and Events during a Residence of nearly Sixteen Years on the Coast of Labrador," by George Cartwright, that in 1785, the systematic destruction of Penguins, had led to the disappearance of these birds, in the breeding season at least, from all their known haunts around the coast of Newfoundland, with the exception of Funk Island:—

"1785. July, Tuesday 5.—A boat came in [to Fogo Harbour] from Funk Island laden with birds, chiefly Penguins. Funk Island is a small flat island rock, about 20 leagues east of the island of Fogo in the latitude of 50° N. Innumerable flocks of sea-fowl breed upon it every summer, which are of great service to the poor inhabitants of Fogo, who make voyages there to load with birds and eggs. When the water is smooth they make their shallop fast to the shore, lay their gang-boards from the gunwale of the boat to the rocks, and then drive as many Penguins on board as she will hold; for the wings of those birds being remarkably short they cannot fly. But it has been customary of late years for several crews of men to live all the summer on that Island for the sole purpose of killing birds for the sake of their feathers, [and] the destruction which they have made is incredible. If a stop is not soon put to that practice the whole breed will be diminished to almost nothing, particularly the Penguins, for this is now the only island they have left to breed upon; all others lying so near to the shores of Newfoundland they are continually robbed. The birds which the people bring from thence, they salt and eat, in lieu of salted pork." *

The same author states that the Red or Wild Indians of Newfoundland visited Funk Island every year.

In 1819 Anspach mentions that, at the time he wrote, the Penguin †

* Vol. iii. p. 55. At page 222 of the same volume, the author writes as follows—and the quotation may serve to show that if such islands as are alluded to were at one time inhabited by Great Auks, the birds may have had other enemies to contend with besides human invaders:—"All along the face of the east coast, and within the many capacious bays which indent it, are thousands of islands of various sizes, on which innumerable multitudes of Eider Ducks and other water-fowl breed. The very smallest are not without their inhabitants, if the spray of the sea does not fly entirely over them; and the larger ones have generally deer, foxes, and hares upon them. The former will swim out to them to get clear of the wolves which infest the continent; but the two latter go out upon the ice, and are left upon them when it breaks up in the spring."

† Pinwing is now the name in use among old residenters, at the various

had been extirpated from its old haunts;* and in 1838, Audubon, who has very little indeed to say regarding the species, and nothing whatever from personal observation, writes as follows:—"The only authentic account of the occurrence of this bird on our coast that I possess was obtained from Mr Henry Havell, brother of my engraver, who, when on his passage from New York to England, hooked a Great Auk on the Banks of Newfoundland in extremely boisterous weather. On being hauled on board it was left at liberty on the deck; it walked very awkwardly, often tumbling over, bit every one within reach of its powerful bill, and refused food of all kinds. After continuing several days on board it was restored to its proper element."† This, as I have already remarked, was probably the last time the Great Auk was seen in that part of the world. The same author also states that when he was in Labrador (no date is given) many of the fishermen had assured him that the "Penguin," as they named the bird, bred upon a low rocky island to the south-east of Newfoundland, and that great numbers of the young were destroyed for bait. Corroborative information had been given him by several individuals in Newfoundland.

From that time until the present day all our information regarding the bird is more or less traditional in its nature. In 1841, howover, ornithologists and others interested in the fate of the species were startled by the announcement made by Peter Stuvitz, a Norwegian naturalist, that he had collected quantities of Penguins' bones on the Funk Islands, and had seen the ruins of the rude stone enclosures into which former visitors had driven the poor birds before being massacred. ‡ Many of these bones are now in the Museum of the University of Copenhagen, and were the first relics

fishing stations on the coast of Newfoundland, who still remember the bird and its odd figure.

* "There was formerly on this coast a species of birds of the diving genus, which from their inability to fly were always observed within the space between the land and the Great Bank, and were once so abundant as to have given their name to several islands on that coast, but they are now utterly extinct. They were known by the name of Penguins."—*History of Newfoundland*, by L. A. Anspach, 1819, p. 393.

† Orn. Biog., vol. iv., 1838, p. 316.

‡ The Zoology of Ancient Europe, by Alfred Newton, M.A., Cambridge, 1862

obtained as corroborative proof of the correctness of the various records by the French sailors whose annual visits and slaughter of the defenceless birds have been already referred to. Shortly before Stuvitz made this discovery, Mr J. B. Jukes, who was engaged in a geological survey of Newfoundland in 1839, thus refers to a group of islands nearer the mainland:—"Aug. 26. At dawn we were under weigh. We sailed along shore as far as Dead Man's Point where the sand beaches ended and a rocky shore began; and then, passing by some low rocks called the Penguin Islands, sailed through the islets called the Wadhams. There was a large island of ice aground off these islands. Penguins were formerly so abundant on these shores that their fat bodies have been used for fuel: they are, however, now all destroyed, and none have been seen for many years."* Writing in the same year in which Mr Jukes' book was published, viz., 1842, Sir Richard Bonnycastle has the following remarks:—"In winter many of the Arctic ice birds frequent the coast, but the large Auk, or Penguin (*Alca impennis*), which not fifty years ago was a sure sea-mark on the edge of and inside the banks, has totally disappeared, from the ruthless trade in its eggs and skin." †

Mention may here be made of a mummified specimen of the bird which was procured from Funk Island in 1863, and forwarded to Professor Newton; and also of three other specimens, preserved in a similar way, from the same locality, which were obtained in the following year. The first formed the subject of a communication to the Zoological Society of London by Mr Newton, and is referred to as, with one exception, the only approach to a complete skeleton existing in Europe; the others passed into the hands of Professor Agassiz, and the British Museum. All these specimens were in a fair state of preservation owing to the antiseptic property of the soil: they were found 3 or 4 feet below the surface, under a covering of ice, about 2 feet in thickness. ‡

* Excursions in and about Newfoundland during the years 1839 and 1840. London, 1842, vol. ii., pp. 115, 116.

† Newfoundland in 1842, by Sir Richard Henry Bonnycastle, Knt., Lieut.-Col. in the Corps of Royal Engineers, vol. i., p. 232.

‡ In "A Short American Tramp, in the fall of 1864" (Campbell), p. 115, the author writes, "About 40 miles outside lie the Funks. Here used to be great numbers of Geyer fogel. Their skeletons are now brought to St Johns with guano."

Following these discoveries chronologically, we now come to the visit of Mr John Milne to the Funk Islands in 1874, an account of which was contributed by that gentleman to the "Field" newspaper, but afterwards published in a separate form. In this paper, entitled "Relics of the Great Auk on Funk Island," Mr Milne has given a most interesting description of the Island and its feathered inhabitants; and as but few persons possess a copy of the pamphlet on account of its scarcity, no apology seems necessary for giving a short extract from the author's narrated experiences on this old dwelling-place of the Penguin.

"At the distance of half a mile the island looked not unlike a smooth-bottomed upturned saucer, slightly elongated into an ellipsoidal form towards its north-eastern extremity, from which end it sloped more gradually up from the sea than it did from its opposite end. As we drew near a few irregularities could be seen along its northern half, which afterwards we found to be heaps of large boulders. Immediately in front of us there was a small cliff, in a crevice of which, we understood, was the usual place of landing. To get ashore at this point—which, as a rule, is the most accessible on the island—is a matter of difficulty. First the boat is rowed alongside the cliff, on the face of which there is a ledge leading to higher ground. The next thing is to balance yourself upon one of the seats or thwarts of the boat whilst it rises, falls, and rolls upon the ever heaving swell. You now wait your chance until you think yourself sufficiently high for a spring. You make it, but it must be without hesitation, and you are landed on your perch. A short scramble, and you are upon the high ground, gazing down at your companions, expanding and contracting as they rise and fall upon the waves, balancing themselves like acrobats whilst waiting to follow your example. However, we were saved from this exhibition of agility by finding a comparatively smooth corner a little further to the north, where, under a shrieking and wailing cloud of birds, one by one we jumped ashore and clambered to a secure foothold."

* * * * * * *

"On the island are several remains of rough stone-work. These, in fact are said to have been used by the now extinct aborigines of Newfoundland, and also by sailors in later times as pens, into which

they drove the Garefowl there to wait until they should be required for use."

* * * * * * *

"Having a strong wish to secure some relics of this bird, and my time for their discovery being limited to less than an hour, it was with considerable excitement that I rushed from point to point and overturned the turf. At nearly every trial bones were found; but there was nothing that could be identified as ever having belonged to the bird, for which I searched. At the eleventh hour the tide turned, and in a small grassy hollow, between two huge boulders, on the lifting of the first sod I recognised an alcine beak. That rare element called luck was in operation. In less than half an hour specimens indicating the pre-existence of at least fifty of these birds were exhumed. The bones were found only from 1 foot to 2 feet below the surface, and in places even projected through the soil into the underground habitations of the Puffins. With the exception of one small tibia and two or three tips of long and thin beaks, probably those of the Tern, all the bones were those of the Great Auk."

* * * * * * *

"In several cases whilst exhuming the skeletons I noticed that the vertebræ followed each other successively, and were evidently in the same position which they occupied when in the live bird. This is in part confirmed by one curious case, where the rootlet of some plant has grown through the neural canal and expanded so at to fix the vertebræ in position. This, together with the fact that there remains no evidence of cuts or blows, leads to the supposition that these birds may have died peacefully. Nevertheless, it may be that they were the remains of some great slaughter, when the birds had been killed, parboiled, and despoiled only of their feathers, after which they were thrown in a heap such as the one I have just described."

Mr Milne also alludes to a considerable difference in size which he observed in several of the bones, but states that the only trace of the bird having been used as fuel was a single burnt fragment of a sacrum. It is, however, possible that with more time at his disposal he might have made further researches which would have thrown some light on traditional records bearing on this subject.

In the last published work on Newfoundland I find the following remarks :—

"The Penguin or Great Auk (*Alca impennis*, Linn.) about seventy years ago was very plentiful on Funk Island, but has now totally disappeared from the coast of Newfoundland. Incredible numbers of these birds were killed, their flesh being savoury food, and their feathers valuable. Heaps of them were burnt as fuel to warm the water to pick off the feathers, there being no wood on the island. The merchants of Bonavista at one time used to sell these birds to the poor people by the hundredweight instead of pork."*

On examining the two eggs which are now upon the table, it will be seen that the word "Egale," written upon each, points to a former French possessor; but if the writing means the name of a rock or islet, I have been unable to trace any such locality on the maps I have consulted, although these are somewhat minute in their indications of both along the entire coast-line of Newfoundland. Professor Newton informs me that on some of the eggs which he has examined the words "St Pierre" are plainly written, which probably signify that such specimens have come from some rocky islet in the neighbourhood of that island, if not from St Pierre itself. This locality is off the southern coast of the group called "Miquelon,"† and is not far distant from the Penguin Islands, visited by Hore and others in the sixteenth century. A glance at any well-prepared map of Newfoundland, including the south-eastern shores of Labrador,‡ will show that all round the coasts there are numerous bays, with rocks and islands bearing such names as Penguin Island, Penguin Isles, Bird Island, Bird Isles, Murr Island, Murr Rocks, Gull Island, Duck Island, Shag Rocks, Cormorant Rocks, Petrel Islands, &c., and we not only infer that all those named "Penguin" were so called on account of the obtrusive numbers of Great Auks frequenting them, but that many of the

* Newfoundland as it was, and as it is in 1877, by the Rev. Philip Tocque, M.A., London and Toronto, 1878.

† Lieutenant Chappell, an intelligent and observant cruiser, in his "Voyage to Newfoundland in H.M.S. 'Rosamond,'" refers to this group (1818), but makes no allusion whatever to the Penguin.

‡ A very good map has been published in the "History of Newfoundland," by the Rev. Charles Pedley (1863). The map accompanying Bonnycastle's "Newfoundland in 1842" may also be consulted with advantage.

others were likewise inhabited by these birds. Even within comparatively recent times Great Auks have been known to occur in some numbers on a rocky islet in Bonavista Bay. Dr William Anderson, late of Brigus and now of Heart's Content, writes to me that Mr Alfred Smith residing at Cupids, and other aged residenters, have informed him that they remember this haunt being partially occupied, and that quantities of the birds were, in olden times, used both as food and fuel. Another informant has stated to him that he remembers seeing, when a boy, Great Auks on the Funk Islands. In sailing past, the birds were pointed out to him as they sat upon the rocks, and the impression he had formed of their size and upright figure had never been effaced. Mr Smith at same time informed Dr Anderson that ten years ago at Manok, or Mannock Island, Labrador, he saw in the hands of some Indians what they spoke of as a young Pin-wing. The length of the bird was equal to that of his hand, and "half-way up the fore-arm." The Indians told him they had picked up the bird dead, but whether on ice, water, or strand he could not ascertain. Dr Anderson, however, whose letter is dated 28th September 1879, cautiously adds that, on further investigation he discovered that these Indians had at the same time a live Porcupine, among other things, for sale or barter, which showed they had been "in the curiosity line."

Another informant—Joseph Bartlett—stated to Dr Anderson that he had often heard his father, who died in 1871 at the age of 70, speak of the Pin-wing; and that crews occasionally got on the Funks, built enclosures, lit fires, and burnt the birds to death for pure mischief. Several other aged masters of fishing-vessels, who have been spoken to by Dr Anderson, recollect perfectly hearing their fathers refer to both birds and eggs which they had taken; and Mr Smith especially referred to the eggs being of "one pint capacity," and the feathers of the bird being of considerable sharpness, readily pricking the skin and causing festering. None of the aged people, however, examined by my correspondent, seem to be able to fix a precise date for the Penguin's disappearance from the Newfoundland habitats.

In alluding to the former existence of the Garefowl in Iceland, I may refer to an excellent paper on the subject contributed by Professor Newton of Cambridge to the "Ibis" for 1861, entitled

"Abstract of Mr Wolley's researches in Iceland respecting the Garefowl or Great Auk."* In this very able paper a graphic and circumstantial account is given of the capture and death of the two latest survivors of their species, which event took place on a rock called Eldey, or Fire Island, by the Icelanders, and by Danish sailors, Meel Sækken, or the Meal Sack, on the 6th June 1844. The chief actors in this memorable undertaking were three Icelanders named Jón Brandsson, Sigurðr Islefsson, and Ketil Ketilsson. Experiencing the greatest difficulty in landing upon the rock, the three men, as they clambered up, saw two Garefowls sitting among the numberless other rock-birds, and at once gave chase. "The Garefowls showed not the slightest disposition to repel the invaders, but immediately ran along under the high cliff, their heads erect, their little wings somewhat extended. They uttered no cry of alarm, and moved with their short steps about as quickly as a man could walk. Jón, with outstretched arms, drove one into a corner, where he soon had it fast. Sigurðr and Ketil pursued the second, and the former seized it close to the edge of the rock, here risen to a precipice some fathoms high, the water being directly below it. Ketil then returned to the sloping shelf whence the birds had started, and saw an egg lying on the lava slab which he knew to be a Garefowl's. He took it up, but finding it was broken, put it down again. Whether there was not also another egg is uncertain. All this took place in much less time than it takes to tell it. They hurried down again, for the wind was rising. The birds were strangled and cast into the boat." And so died the last of the Great Auks. †

The commander of this expedition, on reaching the shore with his ill-gotten booty, started at once for Reykjavik to take the birds to Carl Siemen, at whose instance the expedition had been undertaken; but on his way he seems to have met a knowing purchaser, to whom he sold them for about £9. Allusion is also made in Professor

* Ibis, vol. iii. 1861, pp. 391, 392.

† I have been kindly informed by Mr Wenley of this city, that in July of the present year he had, through the attention of Professor Steenstrup, an opportunity of seeing the remains of these two specimens in the University Museum at Copenhagen. They are simply anatomical preparations, consisting of the intestines and other internal organs—the muscles, bones, skins, and feathers not having been preserved.

Newton's paper to the disappearance of a range of rocky skerries in Iceland, the Geirfuglasker, which was engulphed by the sea in 1830 during a submarine volcanic disturbance, a catastrophe which contributed very materially to the birds' destruction. This range was a noted haunt of the Great Auk for centuries, and the eruption which overwhelmed it seems to have been a final blow towards the extinction of the species.

In connection with Iceland it may not be out of place to refer to the name given to the Great Auk by Niels Horrebow, whose work on Iceland appeared in 1752, viz., the "Geir, or Vulture." Whether this writer had traced any connection between the Iceland name *Geirfugl* and *Lammergeir*, or *geyer* (literally, "lamb vulture"), which is a connecting-link between the Eagle and Vulture, I am not prepared to say—the etymology of the name Garefowl being confessedly a difficult question. Professor Newton informs me that the obvious resemblance at first sight between *Geir* and the German *Geier* or *Geyer* (its older form) has struck several persons, but that he doubts if it is more than a coincidence. The following is Horrebow's account of the Garefowl which I have not seen quoted in any recent publication :—

"The Vulture Rocks, called also Bird Rocks, lie beyond Reikenes in the south district, about 6 or 8 leagues west of this place. On these cliffs and rocks are a great many Vultures, which, besides, harbour in other parts of the island. The inhabitants at a certain season go to these islands, though the expedition is very dangerous, to seek after the eggs of this bird, of which they bring home a cargo in a boat big enough for eight men to row. The danger and difficulty consist in getting ashore near these cliffs, which lie 6 or 8 leagues out at sea, where the water generally runs so high that if the boat be not carefully managed it runs the risk of being dashed to pieces against the rocks by the violence of the waves. Though there are not so many of these birds as of other sea-birds, yet they are not scarce. They are frequently seen; and those that go to take their eggs from them see enough of them. The eggs are very large, and about as big as Ostriches'." Horrebow also quotes the authority of Herrn Johann Anderson, who states that "the Geir, or Vulture, is not often seen in Iceland except on a few cliffs to the west; and that the Icelanders, naturally superstitious, have

a notion that when this bird appears it portends some extraordinary event. Of this he assures us being told that the year before the late King Frederick IV. died there appeared several, and that none had been seen before for many years." * It is worthy of note that in "the new and general map of the island" accompanying Horrebow's work, the Geirfuglasker and Eldey are there marked as "Vulture, or Birds Islands."

It may not be out of place here to refer to the fact of both French and English writers using the term Pingouin or Penguin, in speaking of the Razor Bill (*Alca torda*) as well as of the Great Auk. Thus, Buffon (Ois. vol. ix. p. 393) has given Le Grand Pingouin as the name of the Great Auk, while the Razor Bill is simply Le Pingouin. Temminck (Manuel, vol. ii. p. 937–939) also gives the name *Pingouin brachiptère* and *Pingouin macroptère ;* the former for the Great Auk and the latter for the Razor Bill. MacGillivray (British Water-birds, vol. ii. p. 346) applies the name *Gurfel* to the Razor Bill, while Fleming (British Animals, 1828, p. 130) introduces as Welsh synonymes for the same bird, *Garfil* and *Gwalch y Penwaig.* The name Penguin had apparently at one time been applied in this country to the Razor Bill in popular works, as I find from a map of the Western Isles, published in Edinburgh in 1823, in which it is stated that "the south-west coast of Bernera and Mingulay are remarkably bold precipices rising perpendicularly from the sea in lofty cliffs of gneiss which are frequented in summer by innumerable flocks of Puffins, *Razor-bill Penguins* and Kittywakes. These birds disappear early in autumn with their young."

For the last forty years, if not for a longer period, the money value attached to the eggs and skins of the Great Auk has contributed in a very material degree to the destruction of the species. Caterers for collections, public and private, caused a demand, to supply which organised parties visited the bird's haunts even at the peril of their lives, and effectually exterminated the bird. The very last expedition, as we have seen, resulted in but two birds being captured, and it has a most melancholy interest when we reflect that from that time till now the Great Auk has been a thing of the past. Judging from published records it would seem that there are about seventy skins and about as many eggs of this bird

* Natural History of Iceland, &c., by N. Horrebow, folio, London, 1758.

in existence in European and American collections. That both are of great value no one can doubt. Indifferent skins may be worth from eighty to one hundred guineas, and indifferent examples of the egg more than half that sum. A really fine specimen of the egg, blown at the side, if there be one in existence, must be valued at its weight not in gold, but in bank notes. No wonder, then, that unprincipled persons have been known to imitate both egg and skin —the counterfeit egg especially being a consummate work of art.

Although much has been written of late years about the Garefowl, a complete monograph on this extinct bird is still a desideratum in scientific literature; and ornithologists have for some time been in expectation of seeing such a work produced. The name of Professor Newton of Cambridge University has been mentioned, and naturalists of all nations allow that the task of writing the Great Auk's memorable history could not be committed to abler hands.

5. On a New Telephone Receiver. By Professor Chrystal.

The experiment which forms the subject of this communication was originally devised as an illustration of the explanation of all kinds of microphone receivers, suggested by the beautiful experiments of Mr Blyth, on loose contacts. My idea was to replace Mr Blyth's heated point of metal by a continuous portion of the circuit which should act in the same manner.

It was obvious, for two reasons, that this part must be of small diameter; 1*st*, in order that the resistance, per unit of length, might be great enough to make the variation of the heating sufficient to cause sensible longitudinal extension; 2*d*, in order that the section might be small enough to allow sensible cooling in, say the $\frac{1}{500}$th of a second. I had reason beforehand to believe that the second of these conditions could be fulfilled in practice; because, I found in my experiments on Ohm's Law (Brit. Ass. Rep. 1876, p. 58, *et seq.*) that, when currents of two different strengths alternated, even with great rapidity (60 times per second) in a fine wire, the resistance was sensibly higher during the passage of the stronger current.

My first experiment was tried with Mr Blyth's apparatus. A fine platinum-iridium (10 p. c. Ir. Res. ·7 Ohm per Cm.) wire, about

5 Cm. long, was soldered to a tolerably thick copper wire, which served as a terminal; the other end was fixed securely to the copper spring attached to the mica-diaphragm of the ear-piece. The whole was then put in line with four Bunsen's cells, and a microphone attached to a violin. The experiment succeeded at once. The music was perfectly audible close to the diaphragm, and a tune was reproduced quite distinctly.

For convenience in experimenting with different wires, I constructed the apparatus which I now exhibit to the Society. It consists of a fine palladium-silver wire (4 Ag. 1 Pd. Res. ·52 Ohm per Cm.), 8 Cm. long, soldered to two copper terminals, which are well amalgamated, and lie in the mercury of two cups forming the line terminals. One terminal is hooked to the membrane of a toy drum,* the other end of which is removed; and the other terminal is attached to a string, to which is hung a scale pan, with small weights for producing the requisite tension.

With this apparatus, I can reproduce the music of the violin in the far room, so that all present can hear it. The roughness which mars the effect, is simply due to vibrations of the microphone, which happen to be in unison, now and then, with the note of the violin.

I have satisfied myself that the action of this instrument is not due to loose contacts, or to the earth's magnetism. I believe it to be due to the variations in the heating of the wire, which follow the variations of the current strength caused by the microphone. The tension of the wire does not seem to be material, farther than that there must be a certain tension before the effect is produced; for a wire absolutely loose, gives little or no effect. A thick wire of platinum, with the four cells, did not act until it was made very short; and a wire of copper, 4½ Cm. long and about ·01 Cm. in diameter, would scarcely act at all. The only apparent exception that I found was iron. I found that I could get a tolerable result with an iron wire 4 Cm. long, and thicker than the copper wire last mentioned. A fine steel wire hairspring acted very well, but not so well as the long palladium silver wire. I also tried other metals, but none surpassed the platinum and palladium-silver wires.

So far as I have been able to go with a very fine wire, the effect

* For reproducing articulate speech, a small mica-diaphragm like those used by Edison, Blyth, and others, is best.

increases with the length. There must, of course, be a limit to increase in this way; but, unfortunately, the stock of fine wire obtainable in Edinburgh is neither very varied nor very extensive; so that I could not examine this point farther.

With the same wire, the effect increased with the current strength. The notes came out best when the current just heated the fine wire to a very dull red. It was beautiful then to see the wire burst into a bright glow when reproducing a prolonged note, especially a high one. This glow is sometimes so strong that the wire softens and breaks under the tension. When the wire is shielded from air currents, the glow can be seen to follow the swell of the music, and, with a wire 10 or 12 Cm. long, the motion of the end could be seen quite distinctly keeping time with the swell of the music.

Heating the fine wire *externally* by means of a lamp increases the effect slightly, and cooling with water or a blast of air seemed to produce the opposite effect, but only slightly in the case of very thin wires.

[*Added May* 18*th.*—Since the above experiments were made, my attention has been directed to a paper by Dr Ferguson (Proc. Roy. Soc. Edin., 1877–78, p. 628), in which he anticipates to a certain extent, the main experiment above described. It is true that he has not applied his apparatus to the transmission of music or articulate speech as I have done, but he makes the practically very important step of attaching a mechanical telephone to the wire conveying a varying current, and thereby renders the observation of the sounds of De La Rive both easy and certain. He has also given the very important result that these sounds may be caused by currents of very small total heating effect, such as induction currents. This I have since verified in certain cases.

Dr Ferguson is of opinion that these sounds are not due to heating effects but to some other *molecular* cause, which he does not very clearly define. Except in the case of iron, I see no reason as yet for so explaining them. It must be remembered that it is not the *whole* heating effect that is the question, but its *variation* in a very short time, say the $\frac{1}{500}$th of a second, so that there may be no inconsistency in explaining the ticks in Mr Ferguson's experiments and the music in my own as due to the same cause.

The exceptional behaviour of iron in the case of thick wires (in very thin wires its superiority is doubtful) is not surprising. Professor Tait has suggested a farther anomaly, viz., that at a very high temperature iron may be incapable of producing these sounds altogether.

I am at present in possession of some very interesting results bearing on this point, which I propose to lay before the Society at some early meeting.

It happens, very curiously, that Mr Preece, apparently about the same time as myself, was led to devise an instrument practically identical with the one I exhibited to the Society. An account of it appeared in "Nature," vol. xxii. p. 138; being an abstract of the Proceedings of the Royal Society of London, on May 27th.]

BUSINESS.

The following candidates were balloted for, and declared duly elected Fellows of the Society:—Mr W. F. King, Professor MacGregor, Halifax, N.S., Mr Patrick Geddes, and Dr W. Robert Smith.

Monday, 21st June 1880.

Professor MACLAGAN, Vice-President, in the Chair.

The following Communications were read:—

1. On the Differential Telephone, and on the application of the Telephone generally to Electrical Measurement. By Professor Chrystal.

The plans and calculations in this paper are now more than two years old, but the author has only lately, by the kindness of Professor Tait, found opportunity to carry them out in practice.

A discussion is given of the different methods of applying the telephone to accurate measurement, and mention is made of the points which the author thinks have been missed by most of those who have worked in this way hitherto.

A common principle runs through all telephonic null methods, viz., that the balance may be dependent on the frequency of the interrupted current or may be independent of it. In the latter case

there are in general more than one condition of balance, and in the former case, although one will very often suffice, two may occasionally be necessary. In the former class of cases we get relations between electric quantities of the same dimension, in the latter relations between quantities of different dimensions, *e.g.*, between coefficients of induction and resistances: so that when the frequency is known we can find a coefficient of induction in terms of a resistance, and so on.

A new instrument is described called the differential telephone. It is an ordinary telephone only wound double like a differential galvanometer. The peculiar difficulties attending the construction of an instrument of this kind which will give no sound when the same current passes round its two parallel circuits in opposite directions are explained, and the means of overcoming them pointed out.

The method of using the instrument is explained. A multiple circuit of two branches A and B is inserted in a circuit containing a battery and an interruptor. A and B each contain one coil of the differential telephone, so that the currents pass in opposite direction round it. A and B have self-induction coefficients, M and N, which can be varied at will by altering the configuration of certain coils in the two circuits. If the resistances of A and B be Q and R, then the conditions of equilibrium are shown to be $M = N$, and $Q = R$. There cannot be silence if either of these is unfulfilled, and if both are fulfilled there is silence for all frequencies of the interruptor.

It is pointed out that the instrument, and in fact the telephone generally, is better suited for measuring coefficients of induction than for measuring resistance.

A practical method for procuring a graduated scale of coefficients of induction is then explained.

The mathematical theory of the disturbance of the balance in the differential telephone by two independent circuits E and F neighbouring to A and B is given. If S and T be the resistances, G and H the coefficients of self-induction, and I and J the coefficients of mutual induction with A and B of E and F respectively, then the following four conditions

$$Q = R \qquad SJ^2 = TI^2$$
$$M = N \qquad GJ^2 = HI^2$$

must be satisfied in order that there may be silence for all frequencies of the interruptor.

It is also possible to obtain silence *for a given frequency* by satisfying two conditions, which are given.

The bearings of this theory on practice are pointed out, and the reason explained why the induction balance as arranged by Hughes, does not give results than can be interpreted satisfactorily.

The mathematical theory of the measurement of capacities is then given. If the armatures of two condensers of capacities X and Y be attached, by wires whose resistances may be neglected, to the circuits A and B, so as to include between them all the self-induction of the circuits except that of the telephone coils, it is shown that there cannot be silence for all frequencies unless

$$Q = R, \qquad M = N, \qquad X = Y.$$

Another method is described for finding capacities in terms of resistances. In the circuit A of the differential telephone is inserted a multiple arc, in one branch of which is a condenser of capacity X, the resistance of this branch is Q''. In the other branch there is resistance Q' and self-induction M'. The resistance and self-induction of the rest of A are Q and M, and the resistance and self-induction of B, R and N. The conditions for silence for all frequencies is

$$M = N, \qquad Q' = Q'' = R - Q, \text{ and } \frac{M'}{Q'} = Q'X.$$

This last of these conditions means that the time constants of the coil (M', Q') and the condenser (X, Q') shall be equal. When this is the case, the multiple arc behaves like a resistance Q', having neither induction nor capacity.

It is proposed to apply the differential telephone to the measurement of coefficients of induction, and to the comparison of capacities and their evaluation in absolute measure. It is also expected to prove useful in measuring specific inductive capacity, in investigating the properties of electrolytes, and in examining the internal resistance and polarisation of batteries in action. It is possible that the method last described may afford an improved determination of the ratio of the electrostatic to the electro-magnetic unit.

The rest of the paper is occupied with a discussion of the use of

the ordinary telephone in connection with Wheatstone's bridge. The mathematical theory of various cases likely to prove useful is examined, and their application to the comparison and evaluation in absolute measure of electrical quantities is discussed.

Several curious experiments with the differential telephone were shown to the Society. In particular it was shown that when an interrupted current was passed through one coil of the telephone so that the sound could be heard all over the room, by merely connecting up the parallel coil through a small resistance, the sound was so much deadened as scarcely to be audible at a distance. The deadening effect was shown to be much less when the resistance through which the idle coil was connected had considerable self-induction Grant's experiment ("Phil. Mag.," May 1880, p. 352) was also shown. The electrical theory of these experiments is given in the above paper.

[*Added, August* 19.]

Since the above abstract was written I have carried out the practical application of the above theory much farther. An instrument for giving a variable self-induction with constant resistance has been constructed, and promises to give very good results. I hope to lay a description of it before the Society next winter.

By the kindness of Sir William Thomson and Professor Fleeming Jenkin, my stock of available resistance coils and capacity standards has lately been much increased; and I have been able to satisfy myself as to the thorough practicability of all the above methods of electrical measurement. I reserve details in the meantime; but may mention that the differential telephone shows differences as to self-induction between the so-called induction-less resistance coils. I have measured this difference in the case of two 1000 coils in one of Elliot's boxes, and find it about what might be expected from theory (see Maxwell, "Electricity and Magnetism," vol. ii. p. 291). I believe, from preliminary experiments on capacity measurements with this instrument, that it will be possible by means of it to compare capacities of the order of a microfarad to the $\frac{1}{100000}$ part (provided, of course, that capacity turns out to be definite to that degree of accuracy).

Since I have had the use of standards of capacity I have been

able to add to the general experiments with the differential and ordinary telephone above described. In particular, I have repeated the experiments of Grant in several striking forms, and made a variety of others of a similar nature. I have gone into the mathematical theory of these results, and, I think, succeeded in explaining the curious changes in the quality and intensity of the sounds observed. Among the results, I should desire particularly to draw attention to the theory of the striking alterations produced by condensers in the *pitch*, or, more correctly speaking, *quality* of telephone sounds.

2. On the Determination of the Specific Heat of Saline Solutions. By Thomas Gray, B.Sc., Demonstrator in Physics, and Instructor in Telegraphy, Imperial College of Engineering, Tokio, Japan. Communicated by Professor Tait.

The object of the present paper is to describe the results obtained and the mode of experimenting adopted in some determinations which I have made of the specific heats of solutions of salts. These experiments form part of a series which I am at present carrying out on the physical changes produced when salts are dissolved in different amounts of their solvents. From such an investigation I believe much information may be gained regarding the nature of solution.

The method of experimenting adopted in the experiments described below was that of mixtures; but as regards the mode in which the exact amount of heat added to the solution was measured, it differed from any process with which I am acquainted. This peculiarity consisted in using as heater a thin glass bottle of about 50 cubic centimetres capacity, and furnished with a long glass neck, just wide enough to allow an ordinary mercury-in-glass thermometer to pass through. This bottle was nearly filled with mercury, in which was immersed the bulb of a sensitive thermometer, and thus the temperature of the mercury in the bottle could be read off at any instant. The graduation of this thermometer was to fifths of a degree centigrade, and had been compared with the Kew standards. The distance between two consecutive divisions of its scale was about one millimetre.

The liquid, whose specific heat was to be determined, was placed in a thin glass beaker of about 350 cubic centimetres capacity, which in its turn was contained within a thick porcelain vessel of about two centimetres greater diameter. The two vessels were separated by a packing of cotton wool, which prevented in great measure loss of heat, and at the same time permitted the inside beaker to be removed and replaced with facility. To measure the temperature of the solution a very sensitive mercury-in-glass thermometer was employed. This thermometer, which had also been compared with the Kew standards, was graduated to tenths of a degree centigrade, and the distance between two successive divisions was about one millimetre.

It is evident from what has been stated above that a rise of temperature of one degree could be determined within two per cent. of its true amount, and therefore a rise of temperature of four or five degrees could be measured with great accuracy. There are other causes of inaccuracy, however, than incorrect reading of the temperature, of which the most important is perhaps the variation of temperature in the course of the experiment. All these causes of error were carefully allowed for, and in most cases three experiments made for each density of solution, the arithmetical mean of the results of which was taken as the true specific heat for the solution of the density in question.

The heater was arranged to have about one-tenth of the thermal capacity of the liquid, so that the temperature of the liquid experimented on should not be raised much above that of the atmosphere, and consequently only a small amount of heat be lost by radiation. The method of experimenting was as follows :—

In the first place the thermal capacity of the glass beaker was determined. This was done by filling it to about half the required height with water at the temperature of the atmosphere, and at the same time a similar vessel was filled with water about 10° above atmospheric temperature. The temperature and rate of cooling of this water were accurately determined, and then the temperature of the water in the beaker. The two quantities of water were then mixed, and the temperature read at the end of two minutes and again at the end of four minutes. The difference between these two readings added to the first reading gave the temperature of the

mixture. The differences between this temperature and each of the other two temperatures, viz., the high temperature corrected for cooling, and the low temperature, gave the fall and rise of temperature respectively. Calling now w the weight of water in the beaker, w_1 the weight of water added, t the rise of temperature, t_1 the fall of temperature, and c the thermal capacity of the beaker, we have

$$c = \frac{w_1 t_1 - wt}{t} \quad . \quad . \quad . \quad . \qquad (1)$$

The mean of a set of five experiments made to determine c gave almost exactly 12 as its value.*

The thermal capacity of the heater was next determined. This was done by filling the vessel with the same volume of water as I intended to use of the solutions, and finding the rise of temperature produced in the water by nearly the same change of temperature in the heater as was to be used in the experiments. Putting w' for the weight of water, t' for the rise of temperature, t'_1 for the fall of temperature of the heater, and c' for its thermal capacity, we get

$$c' t'_1 = (w' + c) t'$$

or

$$c' = (w' + c) \, t'_1 \quad . \quad . \quad . \quad . \qquad (2)$$

It is manifestly of great importance that the value of c' should be known with accuracy, and accordingly the experiments for determining it were made with the utmost care. As the following results of a series of experiments do not vary by one per cent. from the mean result, we may conclude that the mean result is true to a fraction of one per cent.

Number of Experiment.	Capacity of Heater.
1	22·521
2	22·402
3	22·457
4	22·501
5	22·373
Mean result, . .	22·451

If now W be the weight of an equal volume of the solution, and

* The mass of this vessel was 70 grammes.

τ its rise of temperature corresponding to a fall τ_1 of the temperature of the heater, then

$$c'\tau_1 = (sW + c)\tau \quad . \quad . \quad . \quad . \qquad (3)$$

where s is the specific heat of the solution. Eliminating c' between this equation and (2) we get

$$\frac{(w+c)t'\tau_1}{t'_1} = (sW + c)\tau \, .$$

If we put $\tau_1 = t'_1$ the last equation becomes

$$(w+c)t' = (sW + c)\tau$$

or

$$s = \frac{(w)t'}{W\tau} + c\frac{t' - \tau}{W} \, .$$

Hence if, as the experiments prove to be the case, the specific heat per unit volume of the solution do not differ very much from unity, t' will be nearly equal to τ, and the value of s will not be materially affected by a small error in the determination of c.

The following is a description of the mode of performing an experiment. The volume of the solution is first roughly measured in a graduated flask, the liquid is then poured into the beaker and weighed. The beaker is then placed in the pad of cotton wool, and the thermometer put into position within it. The heater is placed on a hot plate and heated to about 95° C.; it is then lifted off and shaken up to bring the whole of the mercury to one temperature, and placed in the solution, the temperatures of the solution and of the mercury in the heater being carefully read just before the heater is immersed. Thus all uncertainty as to what the temperature of the heater was when it was placed in the liquid was avoided. The temperature of the solution was noted four or five minutes after the heater was placed in it, the liquid having first been stirred to equalise its temperature. After this interval of time there was no appreciable difference between the temperatures of the heater and liquid. Another reading of the temperature of the solution was made after the lapse of an equal interval of time, and the difference of the two readings added to the first to allow for cooling.

The following table gives the results of experiments on several solutions:—

Name of Solution.	Density.	Sp. Heat.	Mean S.H.	S.H. per unit volume.
Zinc sulphate in water.	1·327	·7453 ·7604 ·7520	·7526	·9987
	1·258	·7723 ·7799 ·7742	·7755	·9756
	1·161	·8586 ·8627	·8606	·9992
	1·075	·9210 ·9467 ·9014	·9230	·9922
Copper sulphate in water.	1·184	·8295 ·8361 ·8405	·8354	·9891
	1·142	·8660 ·8839 ·8864	·8788	1·0036
	1·109	·8883 ·9105 ·9007	·8998	·9979
	1·0871	·9051 ·9222 ·9306	·9193	·9994
Iron sulphate in water.	1·1523	·8450 ·8460 ·8495	·8468	·9758
	1·146	·8792 ·8769 ·8882	·8814	1·0101
Sodium chloride in water.	1·185	·8290 ·8438 ·8441	·8390	·9942
Sodium carbonate in water.	1·080	·9368 ·9248	·9308	1·0053
	1·0893	·9253 ·9230 ·9184	·9222	1·0046
Potassium bichromate in water.	1·0577	·9417 ·9467 ·9538	·9474	1·0021
Lead acetate in water.	1·216	·8217 ·8422 ·8318	·8319	1·0116
Lead nitrate in water.	1·1334	·8704 ·8976 ·8757	·8816	·9992

It is interesting to note the nearness in every case of the value of the specific heat per unit volume to unity. I am continuing my experiments, and hope later to be able to state the results of a more extended series of observations.

3. On a "Navigational" Sounding Machine. By J. Y. Buchanan.

The sounding machine of which the annexed figure is a representation, is intended for use at considerable depths while the ship is proceeding on her course. It is therefore necessarily capable of indicating the depths independently of the length of sounding line or wire used. It possesses the further great practical advantage, that when it has served its purpose once it is immediately available for another sounding.

It indicates primarily the extent to which a given volume of air at atmospheric pressure has been condensed by the column of water to which it has been subjected. As the law of the compression of air is known, the depth reached by the instrument is at once deduced.

The instrument consists of a tube A B, which may either be of uniform diameter, or made up of lengths of different diameters; in the figure the instrument is represented as made of two sizes of tube. This answers all practical requirements. The lower end B is contracted into a nozzle so as to receive a piece of india-rubber tube which can be plugged with a glass rod. At the other end A, a piece of tube drawn out to a moderately fine point is inserted, the point being bent slightly round, and fused hermetically into the end of the tube. The end A is thus closed by a sort of crooked funnel, which on the upper side is contracted to an orifice, at least as small as that of the end of the tube, projecting inwards. The object of this is to sift the water and allow no solid particles to enter which will not pass through the lower orifice of the funnel. The instrument is calibrated either by weighing or by measuring the water which it can

hold. As the volume assumed by the air is inversely as the pressure, it can at once be graduated into fathoms or other units of depth.

For use it is enclosed in a brass tube, and attached to the sounding line and allowed to sink. As it sinks the air is compressed, and its place taken by water which enters through the funnel, and being delivered in a fine stream against the walls of the tube runs down and collects at the lower end. On bringing the instrument to the surface again the water cannot get out, but the compressed air occupying the upper portion of the tube gradually expands through the orifice, which it thus completely occupies and prevents the entrance of water. Arrived at the surface, the depth is read off directly if the instrument is divided into units of depth, or if its scale is arbitrary the depth is found from a table constructed according to the results of calibration. When this has been done the plug C is removed, the water runs out, the plug is replaced, and the instrument is ready for use.

Last summer I had frequent occasion to test the accuracy of one of those instruments in the deep waters of Loch Fyne, and found it most satisfactory. The instrument which I used had an arbitrary scale of equal lengths, and had been very carefully calibrated by weight. From the results of the calibration I constructed a table of depths corresponding to the graduation, on the assumption that it was inversely proportional to the volume assumed by the air, and neglecting any effect of this pressure in causing increased absorption.

The following results obtained on 13th June 1879, while anchored in 87 fathoms off Garrock Head in Frith of Clyde, will show the

Depth (fathoms).	20	40	80
Found	90·5	182·0	243·0
	90·0	183·0	245·0
Mean, . . .	90·25	182·5	244·0
Calculated, ...	89·0	182·6	243·0
Difference, ...	0·25	—0·1	1·0

close agreement between the observed and the calculated depth. The instrument was sent down twice to 20, 40, and 80 fathoms, and

the reading noted. The calculated reading which it ought to have shown on the above assumption is put down, and it will be seen how closely the two agree.

This instrument is especially adapted for surveying-work where lines of sounding in comparatively deep water have to be run for considerable distances, as in the English or Irish Channels, or the North Sea. The sounding line or wire is arranged to pay over the stern, and arrangements are made for promptly heaving it in on bottom being reached. The engines are kept going at a convenient and uniform rate, and the revolution counteracted; it is then easy with good organisation to take soundings every hundred, two hundred, five hundred, or thousand revolutions. In the want of a revolution-counter, which, however, ought to be fitted to every marine engine, equally good results can be obtained if the engineer pays attention to keep his engine going uniformly, checking its rate at frequent intervals by counting the revolutions in a minute, by making the soundings at regular intervals of time. The number of revolutions made by the engines in a vessel of known capabilities is a very accurate means of ascertaining the distance run, and if this method were adopted the deep sounding of a survey could be worked off with great expedition and accuracy.

There is one property of this sounding-machine which must not be lost sight of, namely, that it registers the sum total of the increments of pressure. If it is to give the depth correctly it must both descend and ascend without interruption, and in the work for which it is designed this condition is always fulfilled. Suppose, for instance, it be sunk to 20 fathoms and be then drawn up to 10 fathoms, the corresponding quantity of air will be eliminated. If it is now again sunk to 20 fathoms, the place of the air which left while it was rising from 20 to 10 fathoms, will be taken by water, and if it now be brought to the surface it will of course register a depth greater than 20 fathoms.

It follows from the principle of this instrument that the value of its graduation scale will vary to a certain extent with the barometric pressure. For purposes of navigation the error so caused is negligeable, but if used for surveying purposes a correction must be applied.

The instrument is graduated for a barometric pressure of 30 inches.

As an inch of mercury exercises a pressure equal to $13\frac{1}{2}$ inches of water, we have for the corrected depth

$$D' = D\,\{1 - 0{\cdot}03\,(30 - H)\}$$

where D is the depth read off from the scale, and H is the barometric height. D′ the corrected depth is given in fathoms.

4. On the Compressibility of Glass. By J. Y. Buchanan.

(*Abstract.*)

The experiments related in this paper were undertaken with a view to determine, by actual observation, the effect produced on solids by hydraulic pressure. The instrument used consists of a hydraulic pump, which communicates with a steel receiver capable of holding instruments of considerable size, and also with a second receiver of peculiar form. This receiver consists essentially of a steel tube terminated at each end by thick glass tubes fitted tightly. It is tapped at the centre with two holes, the one to establish connection with the pump and the other to admit a pressure-gauge or manometer. The steel tube may be of any length, being limited only by the extent of laboratory accommodation at disposal. The tube which I am using at present has a length of a little over six feet and an internal diameter of about three-tenths of an inch. The solid to be experimented on must be in the form of rod or wire, and must, at the ends, at least, be sufficiently small to be able to enter the terminal glass tubes, which have a bore of 0·08 inch, and an external diameter of 0·42 inch. The length of the rod or wire is such that, when it rests in the steel tube, its ends are visible in the glass terminations.

The experiment is conducted as follows:—A microscope with micrometic eyepiece is brought to bear on each end of the rod or wire. These microscopes stand on substantial platforms, altogether independent of the hydraulic apparatus. The pressure is now raised to the desired height, as indicated by the manometer, and the ends of the rod are observed and their position with reference to the micrometer noted. The pressure is then carefully relieved, and a displacement of both ends is seen to take place and its amplitude noted. The sum of the displacements of the ends, regard being had to their

signs, gives the absolute expansion, in the direction of its length, of the glass rod, when the pressure at its surface is reduced by the observed amount, and consequently also of the compression when the process is reversed. As, in the case of non-crystalline bodies like glass, there is no reason why a given pressure should produce a greater effect in one direction than in another, we may, without sensible error, put the cubical compression at three times the linear contraction for the same pressure.

The rod experimented on was made of lead glass, drawn by Messrs Ford of Edinburgh, and was 75·05 inches long. The temperature of the water in the hydraulic machine varied from 12·5° to 13·5° C. The pressure varied from 1 to 240 atmospheres. Ninety-one separate observations were made, and the general result is, that the linear compressibility of the glass under experiment is 0·96, and its cubic compressibility 2·92 per million per atmosphere.

5. Suggestions on the Art of Signalling. By Alexander Macfarlane, M.A., D.Sc., F.R.S.E.

(*Abstract.*)

After considering the analogy which exists between the arts of writing and of signalling, the author proceeded to discuss what alphabet is the most suitable where the physical agent is not electricity. If we choose for elementary signals two qualities of the agent of communication A and B, which can be produced independently of one another, then the agent can be put into four states, viz., 1st, having the quality A, but not the quality B; 2d, having the quality B, but not the quality A; 3d, having both the qualities A and B; 4th, having neither of the qualities A and B. One of these states is required to separate letter from letter, and word from word; the fourth state where the agent is undifferentiated, is the one naturally adapted for the purpose. From the remaining three states we can get 3 permutations of one signal, 9 permutations of two signals, 27 permutations of three signals. Without going to a higher permutation than that of three signals, we get 39 symbols,*

* Three of these would probably require to be omitted as repeating the same signal three times.

which are sufficient for the numerals and all the letters contained in the Morse alphabet, less by one. These symbols we suppose assigned to the letters according to their frequency of occurrence as given in that alphabet.

In the case of the Morse system we have only three states of the agent; the third of the above states is not, or cannot be, made use of. As one is required for the purpose of spacing, only two are left to form symbols. To form equivalents for the 39 symbols spoken of above, it requires 2 permutations of one signal, 4 permutations of two signals, 8 permutations of three, 15 of four, and 10 of five. Thus in the former case 102 signals are required to form the alphabet, in the latter 144.

If the elementary signals of the Morse system are made to depend on a difference in quantity, then the above qualitative system possesses other two advantages. Its signals, as they differ in quality, can each be made to occupy the minimum time necessary for a signal to be observed, whereas in the other case the longer signal occupies thrice the time of the shorter; secondly, elementary signals differing in quantity require, though belonging to the same letter, to be separated by an interval, whereas those differing in quality do not.

By assuming that each of the qualitative signals can be sent in the same period of time as the short signals, also that the time required in the Morse quantitative system for the space between the elements of a letter is equal to that period, and the time required for a space between the letters to thrice that period, I have been able to calculate the relative times required by the two systems to signal the words London, Edinburgh, Dublin. The respective ratios are 2·8, 2·6, and 2·9. We may therefore conclude, assuming that the other advantages and disadvantages neutralise one another, that a message can be signalled 2·6 times as quick by the qualitative alphabet described as by the Morse (quantitative) alphabet.

The four-state alphabet would allow one elementary sign to be invariably associated with the right hand, and the other elementary sign with the left hand, and the compound elementary sign with both together. The effect of this would be that a person who had learned to signal by means of any one agent, would have almost equal facility in signalling with any other.

The author then proceeded to offer some suggestions as to how this alphabet could be applied in the respective cases of signalling by means of the heliograph, the light of a lighthouse, steam-whistles, flags, and touch; and advocated the opinion first brought forward by Dr J. A. Russell in a paper read before the Royal Scottish Society of Arts in 1875, that signalling should be taught in the primary schools.

6. Note on the Wire Microphone. By R. M. Ferguson, Ph.D.

At our last meeting Professor Chrystal showed us that a fine platinum wire attached to a stretched disc of skin could act as an electric telephone receiver for the sounds of a violin. The wire was included in a galvanic circuit, and the variations of current were made by a microphone attached to the violin. The account he gave of this interesting experiment was that the receiving wire became extended by the heat of the current either as it was established or suddenly increased by the microphone, and correspondingly shortened on the current ceasing. These extensions and contractions were rendered audible by the disc. A similar demonstration with a like commentary was made by Mr Preece to the Royal Society of London, an account of which was published in "Nature" (June 10). Mr Preece got his wires to speak. At the first May meeting of this Society in 1878 I discussed the subject of the sounds emitted by fine wires, giving passage to intermittent currents. I found that the ordinary thread telephone gave us an easy means of hearing these sounds in non-magnetic metals. De la Rive had heard them in 1845, but since his time no one had been able to hear them, and they were almost looked on as apocryphal. I attached the thread of the skin or paper telephone transversely to the sounding wire, and not directly, as Professor Chrystal has done, for the simple reason that I found that the transverse method gave equally good results with very much less trouble. The cause in both cases seemed to me the same, viz., an internal molecular click which marked the setting in and stoppage of the current. In the kindly reference that Professor Chrystal made to my communication he considered it strange that his simple explanation should have been overlooked, that the sounds should be set down as having conditions the same as those of heat and yet the

simplest and most certain effect of heat passed over. I must confess that the communication deserved that criticism, for the possibility of longitudinal extensions and contractions is not once referred to. At the same time I may say that I thought then, as I do now, that such extensions and contractions are not the cause of these sounds, and the object of this note is to give the ground for such a belief. I may shortly recapitulate why I thought so then.

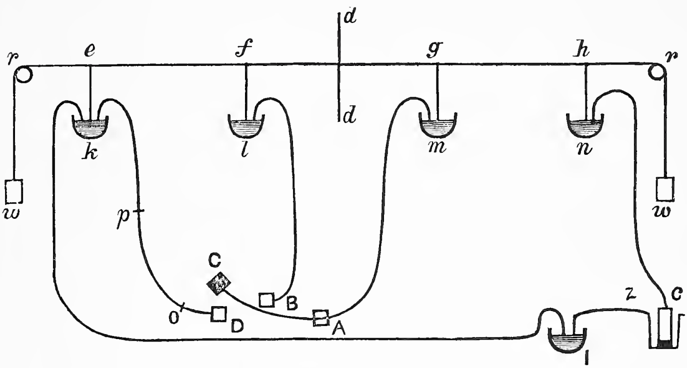
In the first place I was satisfied with the account given by De la Rive. He looked upon the sounds as a magnetic phenomenon. The wire became somehow magnetised and demagnetised by the beginning and end of the current, and the molecules of the wire, in taking and losing the magnetic set, hit against each other and emitted the sounds in question. He thought that such was the case from the exceptional position that iron occupied among the metals he worked with, and from the exactly similar action of wires within a magnetising spiral through which an intermittent current was sent, and those giving direct passage to the same current. The Bell telephone gave me an additional confirmation of this view. Any one who listens to the sounds emitted by discs of different metals when the telephone is excited by a strong discontinuous current and then listens to those given out by wires of the same metals when excited by direct passage of the same current, as revealed by the thread telephone, cannot fail to be struck by the perfect correspondence of the results in both cases. If parity of performance can give any ground for suspecting the same cause, then if the action of the Bell discs be attributed to magnetism, so must also be the action of these sounding wires. This seemed to me at the time convincing enough.

But in addition to De la Rive's magnetic theory, it seemed to me that the sounds originated within the wires, and that they did not need to expend their blows on anything external before sounds were produced. They could be heard when the wires were lying loosely on a table by the aid of a telephone with a wire thread soldered to them. When thrust into the passage of the ear the wires could be heard distinctly without any device for magnifying their loudness. An arc of wire suspended from the thread of a telephone, with each end dipping into adjoining cups of mercury, sounded as loud as if abutting on something solid. Again, it seemed to me unlikely that a wire should receive and divest itself

of its heat as suddenly as it did of its electric charge. A wire takes a sensible time to warm up to the balance of internal gain and external loss of heat and also to lose the heat it has acquired. Now, unless the increments and decrements of length are in the strictest sense momentary, they cannot affect a telephone disc. The clear click that a wire emits when a current begins or ceases in it, both exactly equal in loudness, seemed to me too sharp for the comparatively sluggish course of expansion and contraction by heat and cold. If Professor Chrystal's theory be correct, it can only be the initial increment and decrement of heat that count in this phenomenon, and not subsequent gradual gain or loss. Rapidity of alternation is by no means necessary, for one close or break in a second is as clearly rendered as 500. The extension theory thus appears to me to substitute a thermal for a magnetic onset, and to make it as likely that a sounding clash of molecules accompanies it.

It struck me on considering Professor Chrystal's reasoning on the experiment he exhibited, that if the sound be really in the wire, the skin disc, if placed in the middle instead of at the end of it, would still sound. We should have thus two telephonic receivers, one on each side of the disc. If the sound was due to the push and pull of the wire on getting longer and shorter, the two wires would eliminate the effect the one of the other, but if to internal commotion, little change would be observed whether we had a sounder on one side or on both. So far as I have been able to ascertain the latter is the case. I arranged an experiment in the following way:—*wrefghrw* is a composite thread consisting of very fine platinum wire and fine cotton thread—*ef* and *gh* are the platinum parts. The thread is kept stretched by two equal weights *w*,*w*. The middle of the thread is attached to the skin or paper disc of an ordinary mechanical telephone *dd*. The thread is symmetrically made up on each side of the disc, *ef* and *gh* being exactly equal in every way. At the ends of the platinum wires, wires of copper are soldered which dip into the mercury cups *klm* and *n*, *cz* is a Bunsen battery of six cells. I is a current interruptor or microphone. The one used on this occasion was the mercury break I used for my last communication, vibrating some six times per second. I do not suppose that this deliberate beat, as compared with Professor Chrystal's microphone, which vibrated from 200 to

600 times a second, can affect the result, as what holds for one beat holds for all. I have not beside me a delicate microphone, but so far as I could judge from the rough one in my possession there was no difference in action between the break and it. ABCD is a key the handle of which, by virtue of its elasticity, rests on the stud B, but it can be pressed down on D and thus disconnected from B. The thread is stretched on two rods of glass *rr*, and the telephone may be held in the hand or placed on a supporting board. The current can take two courses according to the position of the handle ABC. As drawn in the diagram it can take the course



*cnhgm*AB*lfek*I*zn*, or if the handle be pressed down, it takes the same as far as A from which it passes to D through K to I and *z*. In the first course both the wires *gh* and *ef* are included, in the latter only *gh*, *ef* being shunted out. To keep the resistance the same in the latter case, a platinum wire *po*, of the same length and thickness as *ef*, is interposed. We have in the first case, as I view it, two opposing receivers on the extension theory or two sounders on the internal click theory. In the second case we have a receiver the same as that of Professor Chrystal. The sound emitted by the disc is much the same in loudness when both wires sound and when only one does; if anything, I fancy it is in favour of the opposing wires. There is also a slight change in sound. When the experiment is so arranged that the wire itself passes through the disc, and we have a continuous wire from *h* to *e*, and when the key is readjusted to take off the current from *m* and lead it back to I, of course through a portion of wire equal to that shunted out

the sound is very much enhanced, arising no doubt from the wire coming in direct contact with the disc. To see if the dipping copper wires had any influence on the sound, I first held them fixed in the hand and found no change. I then took out the mercury and put in a solution of common salt using copper electrodes ; the disc sounded for a beat or two and then ceased, but when the dipping parts were of platinum the beats were quite regular. In this latter case the sound was accompanied by a slight hissing arising from the action of the disengaged gases, but this was quite removed on holding the wires. The sound got when brine or acidulated water is put in the cups is not so loud as with mercury, in consequence of the diminution of current strength.

We do not get the same loudness with this arrangement of the disc in the middle as when only one wire is tightened and the other left loose, for the simple reason that the tightness and consequent elasticity which is favourable to the action of the telephone cannot be so easily got at with a disc balanced between two equal pulls. However, as the common disc is stretched more perfectly the sound rises, so as to leave no doubt, that if the exact tightness were got, the two-pull telephone would be as good as the one with the single pull. Much may be said of the necessary imperfection attending the two-pull telephone. The threads on each side may not be precisely in a straight line, the friction on the rods may not be exactly alike, and that coupled with the comparative fixity of the disc may make a slight difference of pull on opposite sides. But taking all these into account, one would expect that as the conditions approach perfection there would be a corresponding silence, but such is not the case. You may take the telephone into your hand, move it gently about in all directions to secure a position where the loudness is less, but such is not to be found. So far, then, as I can interpret this two-pull telephone, it is something else than the pull of the wire that emits the sound, and the single pull experiment by no means proves the existence of sudden extensions and contractions in the wires. I do not, on the other hand, say that my two-pull experiment disproves them. The conduct of a wire expanding instantaneously in every part may produce an effect like a molecular click, but if so its action must be something very different from the simple pull supposed. A stretched disc with a

stretched thread seems to be capable of rendering all kinds of vibrations as well as those that come normal to its surface.

I also tried if heat or cold on the sounding wires would alter their powers. I endeavoured to apply ice, but unsuccessfully. It was difficult to devise an experiment in which it could be applied and removed so quickly as to produce a contrast. It is also probable that the difference of temperature between that of ordinary air and the freezing point may hardly be great enough to become sensible to the ear. With the Bunsen lamp it was, however, different. It can be easily applied and easily withdrawn. I clamped between two binding screws about $2\frac{1}{2}$ inches of No. 25 platinum wire. I attached a thinner platinum wire to it, which acted as the thread of a parchment telephone pulled transversely. On applying the lamp the sound became sensibly louder and remained so at a white heat. On cooling it again fell off. In the same manner I listened to the effect of a No. 18 soft iron wire. There was a singular increase of loudness just up to the point below which iron became visibly hot, then there was a decided falling off as the wire reached a full red, but it still continued sounding so far as could be judged under the loudness that it had before the lamp was applied. When the lamp was withdrawn the sounds waxed and grew less in reverse order. When the telephone threads were wires of copper and iron the same was observed. When the sound begins to diminish the iron is quite hard, so it is not due to the softening of the iron, which may to some extent account for the falling off at a white heat. In results like these it is possible to discuss the question of molecular impact *versus* expansion, on a new footing. In the case of the platinum the result is much as one would expect, for the rate of increase of its electric resistance and of its expansion are generally allowed to increase at high temperatures. The increase of sound may thus be associated with the one as well as with the other. Iron, however, is exceptional. De la Rive thought that the sounds of wires were in proportion to electric resistance except in the case of iron which stood quite by itself. Here, again, in reference to high temperatures it is quite peculiar. This Society has more than once learned from Professor Tait, and those who have worked with him, of the critical temperature of iron about a dull red heat, in reference to the specific heat of electricity, thermal and electric conductivity,

and anomalous expansion. There is at that point a knot of great complexity and significance. When it is unravelled, there may be something decisive bearing on the present discussion.

Monday, 5th July 1880.

MR ROBERT GRAY in the Chair.

The following Communications were read:—

1. On Peroxides of Zinc, Cadmium, Magnesium, and Aluminium. By J. Gibson, Ph.D., and R. M. Morison, D.Sc.

2. On the Processes in Subepiphysal Bone Growth and some points in Bone Resorption. By De Burgh Birch, M.B., Demonstrator of Physiology in the University of Edinburgh.

(*Abstract.*)

Subepiphysal Bone Growth.—Two processes must be noticed in this connection.

1st, The replacement of the neck of the cartilaginous head or epiphysis by cancellous tissue as an accompaniment to the rise of the epiphysis caused by the growth of the cartilage forming its neck.

The cartilage is channelled by the advancing marrow, the rows of cartilage capsules being opened up.

The opening up of the rows of cartilage capsules results from the presence of a capillary blood-vessel forming the head of the column of marrow which lies in immediate contact with the next unopened capsule (Ranvier). The close proximity into which the pabulum is thus brought with the cartilage corpuscle in the unopened capsule nearest it causes it to grow rapidly and absorb the surrounding cartilage, this occurring, quickest in the direction of least resistance that is, towards the marrow.

The cartilage capsules communicate with each other by means of fine channels, a fact already hinted at by Budge.

The osseous tissue which is deposited upon the cartilage spicules, or septa which result from the channelling of the cartilage, forms the cancellous tissue; this forms a stable base off which the epiphysis

rises by growth of the cartilaginous zone immediately above the primary cancellous spaces.

2nd, The extension of the shaft occurs by opposition to its extremity, thus keeping up with the recession of the epiphysis. The extension of the shaft in length does not lift the head.

The area of proliferation in which these changes occur at the end of the shaft lies in an angular groove at the point where the neck joins the epiphysis, called by Ranvier *encoche d'ossification.*

This author describes fibres in the outer part of the encoche, that is the periosteum, which stretched from the periosteum to the cartilaginous head in which they became lost.

The existence of these is undoubted, and very general.

Origin of the Osteoblasts.—These organisms, which have the function of resorping bone, occur in certain well-marked situations; their origin is from the perivascular connective tissue, *i.e.*, within a short distance of the line of ossification under the epiphysis, and extending over a considerable area, they diminish the number of spicules opening up the cancelli. Under the head of bones, the epiphysis of which have a greater sectional erea than the shaft, and in those positions where the head projects beyond the shaft externally along the interior of the shaft wall.

3. On the Wire Telephone and its Application to the Study of the Properties of strongly Magnetic Metals. By Professor Chrystal.

Four distinct sources of sound were noticed in the course of the experiments.

1. The variation of the longitudinal tension of the wire, owing to variation in the heating, still appears to be the most likely explanation of the action of the wire telephone, when a very fine wire of ordinary metal is used. Experiments were tried with induction coils of various sizes, the violin and microphone being put into the primary circuit and the fine wire telephone into the secondary. It was found that the sound diminished as the spark-giving power of the coil increased. With Professor Tait's large induction coil no sound at all could be obtained, when the secondary was closed through the most sensitive wire I possess.

2. It was found, however, that when the secondary circuit was broken, loud sounds were emitted at the pools of the mercury break. These sounds appear to be due to electrostatic action. They are most probably of the same nature as those obtained in Thomson's singing condenser, Edison's condenser telephone, &c.

3. If the wire of the wire telephone be placed across the lines of force in a strong magnetic field, very loud and pure sounds are obtained when a current interrupted by a tuning-fork is passed through it. These sounds can be obtained with very thick wires of any metal. If a tolerably thin wire be used, although the sound is not much louder, the amplitude of the vibration increases; as much as 2 mm. was observed.

4. Experiments were also made with a view to explain the anomalous behaviour of iron wires established by De la Rive and Dr Ferguson.

Experiments with soft iron wires showed that the sounds did not, in the case of iron, depend in the same way on the length and thickness as they do in the case of ordinary metals, and that their quality is essentially different. The note of the interruptor is often not heard at all, but instead, a variety of other notes are produced, some of them very high accompanied with a fizzing or buzzing noise.

The sound depends on the temperature of the wire, being loudest about a dull red heat, just above the temperature at which the abnormal extension and contraction and the re-glow are usually observed. At higher temperatures the sound falls off very rapidly.

These results suggested that the sound is a consequence of the magnetism of the iron; for, in the case of soft iron, the magnetic susceptibility is at a maximum about the temperature above mentioned, and falls off very rapidly at higher temperatures.

Experiments with steel wires settled the question, for it was found that when the steel was made white hot and then tempered, so as to deprive it of its permanent magnetism and make it hard, it gave no sound at all in the wire telephone. On magnetising it, however, by stroking once or twice with a bar magnet, it sounded quite distinctly, giving a high note and a soft fizzing sound.

The effect of heating a magnetised steel wire is as follows:—At first the sound falls off, first the fizzing disappears, then the high note; then comes an interval of silence; then, as the temperature

increases, the high note comes in again; then the fizzing sound, which quickly rises to a deep buzz accompanied by several notes, among which may be heard the note of the interrupting tuning fork; as the temperature goes on increasing, these sounds die out again in the corresponding order, and when the whole wire is bright red, absolutely nothing can be heard.

All these effects are explained by the magnetism of the steel. The first effect of heat is to destroy the *permanent magnetism*, which about 250° C. is practically insensible; above this temperature the *susceptibility for induced magnetism* increases very fast, reaches a maximum about dull red, and then falls off again.

Advantage was taken of Professor Tait's thermoelectric diagram to verify the close connection between the magnetic, thermoelectric, and other characteristic physical properties of iron and its power of producing sounds, when traversed by a varying current of electricity. The agreement was found to be very striking.

Similar experiments were made with nickel, which is remarkable for the low temperature at which it loses its magnetic susceptibility. The behaviour of a nickel strip in the wire telephone was exactly in accordance with its magnetic properties. The results of thermometric and thermoelectric measurements, rendered the agreement still more remarkable.

Cobalt, when magnetised and heated, gave first a minimum of sound and then an increase; but no maximum was reached at the highest temperature (a bright red), to which I exposed it. This, again, is what is to be expected from its magnetic properties.

Both with cobalt, and with steel which had been softened by heating to a high temperature, the effects due to permanent and to induced magnetism interfere, so that no period of absolute silence appears. Occasionally this interference produces very strong beats. A full account of the experiments above alluded to will be published in "Nature" (vol. xxii. No. 561, p. 303—July 29, 1880).

In the thermo-electric measurement above referred to I had the able assistance of Dr Knott, whose experience in such work is well known to the Society. The curves from which the above references were drawn were constructed by him, and will be given in the detailed account of the experiment to be published in "Nature."

4. Notice of the Completion of the new Rock Thermometers at the Royal Observatory, Edinburgh, and what they are for. By Professor Piazzi Smyth, F.R.S.E.

The nature of this paper may be understood from the following headings :—

(1.) The making and placing of the new thermometers.

(2.) Practically described by Mr Thomas Wedderburn.

(3.) The problem with the old thermometers.

(4.) Their next use in level fluctuations.

(5.) Their employment by Sir Wm. Thomson.

(6.) Their subsequent demonstration of the cycle of supra-annual waves of heat and cold.

(7.) The published predictions in 1872 for 1878–80.

(8.) The spoiled predictions in 1877, under the influence of erroneous sun-spot dates.

(9.) The rectified predictions in 1879, when the true date of sun-spot minimum was ascertained by direct observation.

(10.) How to obtain correct dates for future sun-spot minima.

Appendix I.—The contract for the new thermometers.

Appendix II.—Account of works by Mr Richard Adie.

Appendix III.—Further account by Mr T. Wedderburn, of Adie & Son.

Appendix IV.—The cyclical seasons of 1878–80, as predicted in 1872.

Appendix V.—Scottish meteorological data from 1821–1880, arranged in quadruple annual means for cyclical inquiries.

Plate representing the above numerical tables, graphically.

Monday, 19th July 1880.

Professor Sir WYVILLE THOMSON, Vice-President, in the Chair.

The following Communications were read :—

1. Report on Fossil Fishes collected by the Geological Survey of Scotland in Roxburghshire and Dumfriesshire. Part I. —Ganoidei. By Dr R. H. Traquair.

2. On Some New Crustacea from the Cementstone Group of the Calciferous Sandstone Series of Eskdale and Liddesdale. By B. N. Peach, F.G.S., of the Geological Survey of Scotland. Communicated by Professor Geikie.

(*Abstract.*)

The species enumerated in this paper belong to the two orders Phillopoda and Decapoda.

Of the Phyllopods, the author describes two species of *Ceratiocaris* (Salter), which differ from their Upper Silurian allies in the enormously-developed abdomen and in the small size of the carapace, also in the comparative insignificance of the side spines of the tail compared with the telson. As far as the author is aware, these are the first obtained from the Calciferous Sandstone series, although carapaces of *Ceratiocaris* have been got from the Mountain Limestone of England. (*C. Scorpioides*, $1\frac{1}{2}$ to 2 inches long; *C. elongatus*, 5 to 8 inches long.)

Of the Decapods, seven new species are described, viz., five belonging to the genus *Anthrapalæmon* (Salter), one belonging to the genus *Palæocrangon* (Salter). These do not differ in any essential respect from the recent Macrurous Decapods, and one belonging to *Palæocaris* (Meek and Worthen)—a genus which, as far as the author is aware, has hitherto only been got from the Illinois Coalfield, in the United States, and is represented by only one species, the *Palæocaris typus* (Meek and Worthen). He proposes to call the present one *P. Scoticus*. This is a most interesting creature, for the carapace extends only over the cephalic region, while the thoracic segments are all free and movable, yet its cephalic appendages and the character of the tail show it to be most nearly allied to the Macrurous Decapods.

3. Gaseous Spectra in Vacuum Tubes. By Piazzi Smyth, F.R.S.E., &c.

(*Abstract.*)

The work described in this paper consists of rather careful measurements, but under low dispersion only, of the spectra of twenty gas-vacuum tubes, generally of different gases and illuminated

by small induction sparks, but seen very brightly by the end-on method of viewing.

A comparison of the different spectra thus obtained follows, and some curious results are elicited as to the prevalence of certain impurities among gases, as well as alterations and even transformations of some of them with time and use.

These facts are contained chiefly in Appendix 1 and Appendix 2; while a third appendix, kindly contributed by Professor Alexander Herschel, contains some further observations of his with the same apparatus but higher dispersion introduced. See his account of the same.

Two plates of spectra accompany the paper.

4. On the Diffusion of an Impalpable Powder into a Solid Body. By R. Sydney Marsden, D.Sc., F.R.S.E., &c.

In a note on "The Effect of Heat on an Infusible, Impalpable Powder," by Professor P. G. Tait, in the Proceedings of the Royal Society of Edinburgh, vol. ix. p. 298, for the year 1876–77, Professor Tait points out that such a powder becomes very fluid under the action of heat, and behaves in many respects in the same way as a liquid would do—viz., convection currents are distinctly to be observed, and small particles of the powder are thrown up from the surface, in the same manner as we perceive little drops of water thrown up from the surface of a glass of soda-water. And Professor Tait then asks the question—If, supposing we had two such infusible, impalpable powders, would they diffuse into one another as do gases and liquids? This is a question which as yet has not been answered. Professor Tait and I have been engaged in some experiments on the subject for some time, but the difficulties (chemical and physical) to be overcome are much greater than at first sight appear, and at present we are unable to say definitely whether they do so or not. But I think an answer may be obtained from another source. In some recent experiments I had occasion to have a number of Berlin porcelain crucibles and amorphous carbon in an impalpable powder kept in contact with each other at very high temperatures for from ten to twelve hours, with the following effect, that, although the crucibles did not become fused, but

retained their exact original form, yet the carbon found its way to a considerable distance into the crucible, and some of the particles penetrated the crucible throughout. This was not a case, therefore, of fusion and mechanical mixture.

On examining a section of the crucible under the microscope, the particles of carbon can be distinctly seen disseminated through the silica and alumina of the crucible, and thickest in the glaze and outer parts which are nearest to the carbon. We also notice a number of "tricites" all along the juncture of the alumina with the glaze of the crucible, arising from the devitrification of the glaze, and a number of particles of larger size which are contained in the original crucible. On examining a section of the crucible before being used we see nothing in the form of little black particles disseminated throughout, and are thus able to recognise more completely these black specks as carbon, the result of diffusion.

Now carbon, so far as we know, has no chemical action on silica or alumina, and consequently it cannot have been taken into the crucible by chemical action. This, then, is a distinct case of the diffusion of an impalpable powder into a solid body in a softened state. And it has the advantage that, the solid body being transparent, we can, by examining it with the microscope, see what has actually taken place. And here it gives us an insight into another matter. It is evident that this is precisely what takes place in the conversion of bar iron into steel by the cementation process. The carbon in the state of an impalpable powder diffuses into the bars of iron whilst they are in the softened state, the operation taking a number of days before it is completed. Thus it seems to me to explain the up to the present time unsettled question of the conversion of iron into steel by the cementation process, and to render unnecessary the "Occlsuion of Gases" theory.

In order to make absolutely certain of the fact that carbon had really penetrated into the crucible, I took a portion of the crucible, pounded it down, and treated it with hydrofluoric acid for some days. I then filtered off the insoluble residue which was left, and after treating it successively with hydrochloric acid and soda, ultimately, on largely diluting, got the carbon (in an exceedingly fine state) suspended in the water, and by decantation, and filtering the decanted fluid, got it on to a filter. Its quantity, however, in this

extreme state of division was not large enough for me to get it off the filter and examine it further; but after the treatment which it had received—it still remaining a brownish-black powder—there can be no doubt of its being carbon.

A similar case of diffusion takes place on a small scale when we hold a cold porcelain lid over a bunsen flame, when, as is well-known, we obtain a black deposit under the glaze of the porcelain without the latter being fused. Here the carbon in the impalpable condition diffuses itself into the porcelain, but aided by the convection currents of the gases of the lamp.

5. On the Variation with Temperature of the Electric Resistance of certain Alloys. By Professor J. G. MacGregor and C. G. Knott, D.Sc.

6. Preliminary Report on the Tunicata of the "Challenger" Expedition. Part II. By W. A. Herdman, D.Sc.

(*By permission of the Lords Commissioners of the Treasury.*)

Since the publication of the first part of this preliminary report (Proc. Roy. Soc. Edin., 1879–80, p. 458), I have received some additional specimens of Ascidians belonging to the "Challenger" collection, and including the following Ascidiadæ.

Ascidia cylindracea, n. sp.

External appearance.—Shape nearly cylindrical; posterior end rounded and wider than truncated anterior end; ventral edge nearly straight, dorsal slightly concave. Attached by base and lower half of left side. Both apertures at anterior end; branchial towards ventral side, sessile; atrial on dorsal edge, forming a rounded projection; both distinctly lobed. Surface smooth. Colour yellowish-grey. Length, 2 cm.; breadth, 1·2 cm.

Test of moderate thickness, transparent, showing vascular ramifications.

Mantle having well-marked muscular bands.

Branchial sac extremely delicate; vessels very slender. Stigmata long and narrow, some being twice as long as others in consequence

of the alternate transverse vessels being interrupted here and there, and sometimes altogether wanting. Internal longitudinal bars narrow but well-marked, having papillæ at the angles of the meshes; generally three stigmata in a mesh.

Tentacles very long and numerous, their bases almost touching.

Dorsal lamina plain; no ribs or teeth.

One specimen from Station 163 (Twofold Bay, Australia); 120 fathoms.

Ascidia despecta, n. sp.

External appearance.—Shape oval; the anterior end being narrow while the posterior is wider and rounded. Dorsal edge rather more convex than ventral. Attached by posterior half of left side. Branchial aperture near anterior end; atrial not distant, on dorsal edge about one-fourth of the way down. Surface covered with small soft projections giving a rough appearance. Colour grey. Length, 1·7 cm.; breadth, 1 cm.

Test thin, nearly transparent, showing fine vascular ramifications. Trunks enter near centre of area of attachment. Test prolonged into a few short tufts near base of left side.

Mantle normal.

Branchial sac not plicated, rather stout. Internal longitudinal bars strong, bearing large papillæ at the corners of the meshes, no smaller intermediate ones; three or four stigmata in a mesh.

Tentacles large and numerous, all one length.

Dorsal lamina wide, transversely ribbed; margin plain.

One specimen from Kerguelen Island; 10 to 100 fathoms.

Ascidia nigra, Savigny.

Three specimens from Station 142? (south of Cape of Good Hope); 150 fathoms.

Ascidia pyriformis, Herdman.

A large specimen from Port Jackson; 6 fathoms.

Ascidia placenta, n. sp.

External appearance.—Shape elongate, elliptical or oval; flattened laterally; the anterior end slightly the narrower, posterior

end rounded. Attached by a small area a little posterior to the middle of the left side. Apertures both on right side, inconspicuous, sessile: branchial median and nearly terminal; atrial a short distance from the dorsal edge, more than one-third of the way down, lobes indistinct. Surface slightly wrinkled and approaching to velvety. Colour yellowish-grey or horn-colour. Length, 6·5 cm.; breadth, 4 cm.

Test rather thin, soft, easily torn, roughish about base of attachment. Inner surface smooth and glistening; vessels feebly developed.

Mantle moderately muscular.

Branchial sac very delicate, minutely plicated; stigmata long and thin, eight to twelve in a mesh. Papillæ long and curled; smaller intermediate ones also present, and in some places connected by fine transverse vessels.

Dorsal lamina slightly ribbed transversely, with a large tooth at the end of each rib and three or four smaller intermediate ones.

Tentacles filiform, about 24 in number, all the same length.

Olfactory tubercle longish elliptical, with the opening at the anterior end.

Two specimens from Station 150 (south of Kerguelen Island); 150 fathoms.

This species resembles *Ascidia tenera* considerably in external appearance, but is quite distinct.

Corella japonica, Herdman.

Three specimens from Kobé, Japan; 8 to 50 fathoms.

II. Clavelinidæ.

The little group of Social Ascidians is here placed next to the Ascidiadæ as a fourth family of *Ascidiæ simplices.* The old name Clavelinidæ is retained, the only change being that, instead of occupying a position intermediate between the Simple and Compound Ascidians, they will now be included in the former group. As the explanation of my reasons for making this change in classification necessitates frequent reference to former observations and theories, it is simpler, and seems more advantageous, to give the argument in the form of a brief outline of the history of the group.

The first Social Ascidians known to science were two species of *Clavelina*, viz., *C. borealis* and *C. lepadiformis*.

The first of these, *Clavelina borealis*, was described under the name *Ascidia clavata* by Pallas* in 1774, and was referred to by Bruguiere† in 1789. It was afterwards, in 1815, described at greater length under the same name by Cuvier,‡ who united it with *Ascidia* (now *Ciona*) *intestinalis* to form his fourth tribe of the genus *Ascidia*.

The second species, *Clavelina lepadiformis*, was observed by Müller§ and described by him in 1780 under the name of *Ascidia lepadiformis*; Bruguiere|| (1789) mentions this species also.

In 1816 Savigny¶ founded the genus *Clavelina* for the reception of these two species, which he separated from *Phallusia* (*Ascidia*) on account of their being pedunculated. He still retained them, however, in the Simple Ascidians. In his third memoir (p. 109) he gives an account of *Clavelina borealis*, and states (p. 110) that "Les véritables rapports des Clavelines sont avec les Phallusies." In his systematic table (p. 171) he places *Clavelina* as the last genus of the Simple Ascidians next to *Phallusia*, and immediately following the *Phallusiæ Cionæ* (*C. intestinalis*).

In the same year (1816) Lamarck** places these two species, *C. clavata* and *C. lepadiformis*, in the genus *Ascidia*.

It is evident, then, that those of the older naturalists to whom any of the Clavelinidæ were known included them unhesitatingly in the Simple Ascidians. It must be remembered, however, that although Gaertner was acquainted with *Botryllus* and *Distomus* in 1774, and Renieri (1793) was to a certain extent aware of the true nature of some of these forms, yet the Compound Ascidians were hardly recognised as such till after the appearance of Savigny's well-known memoirs. By Cuvier, Savigny, and Lamarck, however, to all of whom the Compound Ascidians were well known, *Clavelina* was considered a Simple Ascidian closely allied to *Ciona intestinalis*.

* Spicilegia Zoologia, fasc. 10, pl. i. fig 16.
† Encyclopédie méthodique, pl. lxiii. fig. 11.
‡ Mém. du mus. d'hist. nat., t. ii. pl. ii. figs. 9, 10.
§ Zoologia Danica, Pt. ii. p. 119., tab. lxxix. fig. 5.
|| *Loc. cit.*, pl. lxiii. fig. 10.
¶ Mémoires sur les animaux sans vertèbres, Pt. ii., fasc. 1, p. 87.
** Histoire naturelle des animaux sans vertèbres, t. iii. p. 126.

In 1834, J. J. Lister, F.R.S.,* published a paper entitled "Some Observations on the Structure and Functions of Tubular and Cellular Polypi and of Ascidiæ," in which he gave an account of a small species of Ascidian, afterwards described by Wiegmann† as *Perophora listeri.*

Lister pointed out the condition in which the individuals lived, the fact that each possessed a complete set of organs of its own, but that all were connected by a common circulatory system; and stated that "it increases by sprouts: the two streams of the stem run through the bud before its organs are developed."‡

Clavelina remained in the *Ascidiæ simplices* till 1842, when Milne-Edwards published his celebrated "Observations sur les Ascidies composées des côtes de la Manche."§

In this elaborate work he gives an account of several species of *Clavelina*, and proposes that that genus, along with *Perophora*, should be separated from both Simple and Compound Ascidians, and form an independent intermediate group, to which he gives the name of *Ascidiæ sociales.* This group he defines as comprising ascidians which reproduce by buds as well as by eggs, and which live united by common radiciform prolongations, but which otherwise are free of all adhesion to one another. "On réserverait alors le nom d'*Ascidies simples* pour les Ascidies qui ne se reproduisent point par bourgeons, et qui ne vivent pas réunies en groupes, par l'intermédiaire d'une portion commune du tissu tégumentaire.

Enfin, les *Ascidies composées* se rapprocheraient de cette division nouvelle par leur mode de multiplication, mais s'en distingueraient par l'existence d'un seul corps tégumentaire commun à tous les individus dont se compose chaque colonie; tandis que chez les premiers, chaque individu possède une tunique tégumentaire qui lui est propre."||

Milne-Edwards' ground for separating the Social from the Simple Ascidians was twofold; first the union of the individuals by stolons, and secondly the power they possess of reproducing by gemmation. Of course these two points are really only one, as the union is

* Phil. Trans., 1834, Pt. ii. p. 365.
† Wiegmann's Archiv, 2 Bd., 1835, p. 309.
‡ *Loc. cit.*, p. 382.
§ Mém. Inst. France, vol. xviii. p. 217.
|| *Loc. cit.*, p. 266.

simply the result of the gemmation, and taken alone is not a characteristic of any importance.*

The power of reproducing by gemmation is of more value, and seems at first sight to form a distinction between the CLAVELINIDÆ and the other Simple Ascidians; this, however, is more apparent than real. The buds on the stolons of the CLAVELINIDÆ are developed from the ends of the blood-vessels, and are at first merely slight enlargements similar to and comparable with the knobs on the end twigs of the vessels in the test of an *Ascidia;* these last vessels being homòlogous with those in the stolons of the *Clavelina.* In *Ascidia* the vessels do not project beyond the test, but in *Molgula* they are prolonged considerably as hair-like simple or branched processes,† and in *Ciona*, at the base of the test, projections exactly like the stolons of *Clavelina*, having the same structure and containing similar blood-vessels, frequently grow out over the object to which the individual is attached.

It thus appears that all the apparatus necessary for budding is present in the Simple Ascidians as well as in the so-called Social, and that in the former it may even go the length of forming stolons, but these have never been seen to develop into new individuals.

Philippi ‡ in 1843 gave a short account of an Ascidian he had found at Naples, and which he called *Rhopalæa neapolitana.* This form is elongated, somewhat like a *Clavelina* in shape; the branchial aperture, however, is eight-lobed, and the atrial six-lobed as in *Ascidia.* "Im obern Drittheil etwa, wo die Verdickung anfängt merk licher zu werden, sassen in einem unregelmässigen Kranz zweispaltige und dreispaltige Auswüchse, jungen Ascidien nicht unähnlich." The body is divided into thorax and abdomen joined by a narrow neck; the heart is placed on the right side of the intestinal loop, and the

* If the mere fact of the union of individuals, irrespective of the cause of that union, is to be considered an important point, then aggregations of true and undoubted Simple Ascidians of the genera *Ascidia* and *Cynthia* must also be considered colonies of Social Ascidians, as they were in the case of *Styela grossularia* by Van Beneden in 1847 (Mém. de l'Acad. roy. de Belgique, t. xx.). It is now well known that these aggregations are merely caused by the proximity and the coalescence of the tests, and indicate no relationship whatever between the different individuals.

† For an explanation of the true nature of these hair-like processes in the Molgulidæ, see Lacaze-Duthiers, Arch. de Zool. expér. et gén. vol. iii. p. 314 (1874).

‡ Müller's Archiv für Anatomie, 1843, p. 45.

ovary on the left just as in *Clavelina*. The branchial sac, however, is provided with strong papillæ. The dorsal lamina finally is formed of languets.

This is a very interesting form, being clearly intermediate in its characters between *Clavelina* and *Ciona*. The external shape and the condition of the dorsal lamina would allow of its being placed in either genus. The presence of thorax and abdomen, and the position of the heart and ovary, ally it to *Clavelina*, while the lobes round the branchial and atrial apertures, and the papillæ on the branchial sac, show its relationship to the ASCIDIADÆ. Finally, the bud-like projections from the test, a careful investigation of which would have been valuable, seem in the figure very like young individuals, and, if they are so, indicate gemmation probably from the blood-vessels of the test.

Adams * in 1858 placed Philippi's *Rhopalœa* in the old genus *Clavelina*. This, I think, is quite wrong. *Clavelina* has no lobes round its apertures, and no papillæ on its branchial sac, while *Rhopalœa* has both; still the fact showed that the resemblance of this form to the CLAVELINIDÆ was detected, although it had been described by Philippi as a Simple Ascidian.

Bronn † (1862) follows Milne-Edwards, and divides the *Ascidiæ* into *simplices*, *sociales*, and *compositæ*.

Claus ‡ (1876) unites the ASCIDIADÆ and the CLAVELINIDÆ as one group, "*Einfache und aggregirte Ascidien*," distinct from the compound Ascidians (*Zusammengesetzte Ascidien*).

Professor Giard in his "Recherches sur les Ascidies Composées ou Synascidies" § (1872) unites the Social Ascidians with the Compound, including *Clavelina* and *Perophora* in the *Synascidiæ*. This he does chiefly on account of their property of budding, although he admits that budding alone is not sufficient to separate the Social from the Simple Ascidians. He gives as the characteristics of his *Synascidiæ* ||:—Reproduction by gemmation, stigmata oval, the embryo being developed rapidly, and being almost complete before being hatched.

* Genera of Recent Mollusca, vol. ii. p. 595.

† Klassen und Ordnungen des Thier-Reichs, B. III. p. 216.

‡ Grundzüge der Zoologie, p. 840.

§ Arch. de Zool. expér. et gén., vol. i. p. 501.

|| *Loc. cit.*, p. 603.

The first of these characters he had admitted to be insufficient alone, and I am unable to recognise the value of the other two. It is difficult to understand how the shape of the stigmata can be a characteristic of much value, and the statement that oval stigmata are characteristic of the *Synascidiæ* may be easily refuted, as many Simple Ascidians have oval stigmata, while a species of *Colella* (*Aplidium pedunculatum*, Q. and G.), an undoubted Synascidian, has very long slit-like stigmata with parallel sides. The stage of development in which the embryo is hatched cannot be considered of much importance, as it seems to vary in closely-allied forms; and Giard's generalisation that only in Compound Ascidians does the embryo remain in the egg-membrane till far advanced in development, will certainly not hold, as Kupffer * describes and figures the embryo of *Molgula macrosiphonica* as being still, when almost completely developed, covered by the "eihaut." This is also the case in several others of the few Simple Ascidians, the development of which has been observed.

In conclusion, it appears that the power of reproducing by gemmation is the only difference of more than generic rank between *Clavelina* and *Ciona*, while *Ecteinascidia*, a new genus of the Clavelinidæ might, were it not for the fact that it reproduces by budding, and that the individuals are united into colonies, be included in *Ciona*.

In *Clavelina* in an adult colony, the stolons connecting the bases of the individuals often atrophy and in places entirely disappear, leaving the individuals without any connection. They are now practically Simple Ascidians. In *Ecteinascidia*, the same seems to happen; and I believe that if a *Ciona intestinalis*, a solitary *Clavelina lepadiformis*, and a solitary *Ecteinascidia turbinata* were submitted to a naturalist who did not know the several species, he would declare that they were all Simple Ascidians, that the *Ecteinascidia* and the *Ciona* were species of the same genus, and that the *Clavelina* was a nearly allied one. This would be the natural arrangement were it not for the budding, which, however, should be considered of sufficient importance to characterise a family, and, therefore, I unite those Simple Ascidians which reproduce by gemmation and form colonies, including the genera *Clavelina*, *Ecteinascidia*, *Perophora*

* "Entwickelung der einfachen Ascidien," Archiv für Microscopische Anatomie, 1872.

and possibly *Rhopalœa*, as the family Clavelinidæ, and place them next to the Ascidiadæ in the *Ascidiæ simplices*.

CLAVELINIDÆ.

- Branchial sac having internal longitudinal bars.
  - Internal longitudinal bars bearing papillæ. — *Rhopalœa.*
  - Internal longitudinal bars having no papillæ. — *Ecteinascidia.*
    - Test very thick. — *E. crassa.*
    - Test thin, at least on thorax.
      - Tentacles few, all one length. — *E. fusca.*
      - Tentacles about 80, of three lengths. — *E. turbinata.*
- Branchial sac having no internal longitudinal bars.
  - Branchial sac not papillated. — *Clavelina.*
    - Horizontal membranes round branchial sac narrow. — *C. oblonga.*
    - Horizontal membranes round branchial sac very wide. — *C. enormis.*
  - Branchial sac papillated. — *Perophora.*

Ecteinascidia, n. gen.

External appearance.—Shape oblong, tapering posteriorly.

Branchial sac having internal longitudinal bars, but no papillæ.

Dorsal lamina reduced to languets.

Tentacles simple.

Viscera extending beyond branchial sac posteriorly.

This genus is formed for the reception of three species which seem to be intermediate in their characters between *Ciona* and *Clavelina*, and, except in one point, resemble Philippi's *Rhopalœa* more than any hitherto described form.

Ecteinascidia must, on account of its property of forming colonies by gemmation, and having no papillæ on its branchial sac, be included in the CLAVELINIDÆ, but it differs from *Clavelina* in possessing well-marked internal longitudinal bars. In this last character it approaches *Ciona* and *Rhopalæa*, from both of which it differs in the absence of papillæ.

Ecteinascidia, crassa, n. sp.

External appearance.—Shape irregular, rudely triangular; attached by extended base to clump of sponge spicules. Anterior end more or less rounded; sides irregular. Both apertures sessile, near or at anterior end. Surface rather irregular. Colour yellowish-grey. Length, 2 cm.; breadth along base, 3·5 cm.

Test enormously thickened.

Mantle strongly developed. Muscle bands thick.

Branchial sac crumpled. Internal longitudinal bars fine, undulating, borne on large pyramidal ducts. No papillæ. Stigmata elongated.

Dorsal lamina languets.

Viscera extending considerably beyond branchial sac, and forming a distinct abdomen.

Two specimens attached to the spicules of a large sponge (*Laburia hemisphærica*) from Station 192 (Ki Island); 129 fathoms.

Ecteinascidia fusca, n. sp.

External appearance.—Individuals united by a short, thick, irregular stolon, which looks merely like a continuation of their posterior extremities. Shape very elongated, some specimens rudely club-shaped; anterior end wide, truncated; posterior half narrower, contorted, passing down into the stolon. Apertures nearly terminal, both placed on the right side of the extremity; branchial near the middle; atrial near the dorsal edge. Surface smooth but uneven, especially at the posterior end, where knobs and processes are formed. Colour dark brown. Length, 5 cm.; breadth, 1·5 cm.

Test thickish, especially on the posterior part; vessels present.

Mantle thin; muscular fibres distant, but well marked, and of a reddish-brown colour.

Branchial sac delicate. Internal longitudinal bars narrow but distinct, undulating, supported by broad horizontal membranes, provided with triangular flaps, to which they are attached. No papillæ. Stigmata longish, elliptical; about three in a mesh.

Dorsal lamina reduced to languets.

Tentacles filiform, few and distant.

Olfactory tubercle irregularly oval-shaped.

Viscera prolonged posteriorly to the branchial sac, and extending into the posterior narrow part of the body.

One colony (several individuals) from Banda; 17 fathoms.

Ecteinascidia turbinata, n. sp.

External appearance.—Many individuals united by a delicate much-branched stolon. Shape elongated, sometimes almost pyriform; anterior three-fourths of much the same width, posterior end narrowing rapidly to a short slender stalk continuous with the stolon. Both apertures on the right side of the anterior end, sessile. Surface smooth. Colour light yellowish-brown. Length, 3 cm.; breadth, 1 cm.

Test thin and membranous, transparent.

Mantle thin.

Branchial sac simple. Internal longitudinal bars narrow, but well marked. No papillæ. Stigmata elliptical, rather long.

Dorsal lamina reduced to languets, small and rather distant.

Tentacles filiform; of three lengths, placed alternately, about twenty of the long and medium sizes and forty of the short. They are placed thus:—long, short, medium, short, long, &c.

Olfactory tubercle elongated posteriorly, so as to be carrot-shaped.

Viscera extending slightly beyond the branchial sac posteriorly.

One large colony from Bermuda. Shallow water.

Clavelina oblonga, n. sp.

External appearance.—Individuals closely united by their posterior extremities, which form thick irregular stolons. Shape irregularly oblong, or sometimes club-shaped; anterior end wide and rounded;

posterior generally very narrow. Both apertures at anterior end, sessile, not lobed. Surface smooth, with occasional transverse wrinkles, especially on posterior end. Colour light yellowish-grey, nearly white. Length, 2 cm,; breadth, ·7 cm.

Test thin, especially at anterior end; transparent.

Mantle moderately strong.

Branchial sac simple, transverse vessels of one width, bearing horizontal membranes; no internal longitudinal bars; stigmata short, elongate-elliptical.

Dorsal lamina.—Languets of moderate size.

Tentacles short and stout, about twenty in number, alternately long and short.

Olfactory tubercle small, irregularly oval in outline.

One colony from Bermuda. Shallow water.

Clavelina enormis, n. sp.

External appearance.—Individuals united by their bases to form an irregular mass. Shape rudely oblong, with both apertures at anterior end. Surface smooth but irregular, especially on the posterior part. Colour greyish, with a slight brown tinge. Length, 3 cm.; breadth, ·7 cm.

Test moderately thin on the anterior half; posteriorly thicker, wrinkled, and encrusted with sand.

Mantle well developed.

Branchial sac.—Transverse vessels all one size, with wide horizontal membranes hanging from them. Stigmata regular, short, and narrow, with rounded ends. Fine longitudinal vessels (interstigmatic) strong.

Dorsal lamina.—Languets large, close, and numerous.

Tentacles stout, long and short alternately; about twelve of each.

A single colony of four adult individuals and several buds. The united posterior ends form an irregularly-shaped base, which adhered to the surface of a mass of *Balani*, *Synascidiæ*, &c. Two of the adult individuals are united together along one side, so that their tests form a common investing mass.

I am convinced that this is a pathological specimen, that the adhesion of the two individuals is abnormal, that the irregular

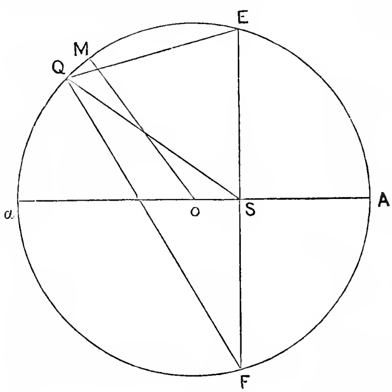
stem-like base is a hypertrophy due to the irregular surface the colony was attached to, and that, therefore, this species cannot with certainty be separated from *Clavelina*.

Simon's Bay; 10 to 20 fathoms.

7. Description of New Astronomical Tables for the Computation of Anomalies. By Mr Edward Sang.

(*Abstract.*)

The planets move round the sun in ellipses, in such a manner that the radii vectores describe areas proportional to the times. Now, by means of parallel lines, we can always project an ellipse upon a plane surface so as to make the projection circular, and thus we have to consider the motion of a point in the circumference of a circle, describing round an excentric point areas proportional to the times. If we take S for the excentric point, that is for the projection of the sun, and suppose Q to be the projection of the planet's place, the area ASQ is proportional to the time elapsed since the perihelion passage. The angle AOQ is called, very inappropriately, the excentric anomaly; I prefer to call it the angle of position. If we suppose a point M to move uniformly along the circumference, with the periodic time of the planet, and to have reached M when the actual projection of the planet is at Q, it is clear that the sector AOM must be equivalent to the area ASQ. The angle AOM is the mean anomaly.



Having drawn ESF perpendicular to the diameter ASOa, join QE and QF; then it is evident that the surface EQFA is halved by the compound line ASQ; wherefore the area ASQ passed over by the radius vector is half the sum or half the difference of the circular segments QAF and QE, according as Q lies beyond AE or within it.

Denoting the arc AE by e, and the arc of position AQ by p, and

observing that the area AOM is equivalent to ASQ, we have, denoting the segment AOM by m,—

$$2m = \text{segm. } (p+e) + \text{segm. } (p-e),$$

and thus the determination of m from p, or of p from m, is to be accomplished by help of a table of circular segments, which must be measured, not in parts of the square of the radius, but in degrees of the surface of the circle.

For the purpose of rendering this exceedingly simple formula available for actual calculation, a table was constructed of the sines for each minute of the quadrant, measured in degrees of arc; by its help the values of the circular segments for each minute of the whole circumference were written out, true to within one ten-thousandth part of a second of the modern division.

When we have got a tolerable first approximation, this table enables us to compute the position corresponding to a given mean anomaly by a simple proportion.

In order to guide us to a first assumption, tables were constructed of the mean anomalies corresponding to each degree of position from 0° C. to 200° C., and for every value of e from 0° C. to 100° C., with their differences and variations, true to the nearest second; and thus, in every possible case, the solution of Kepler's problem is obtained in a few minutes, true to far within the hundredth part of a second of the new division.

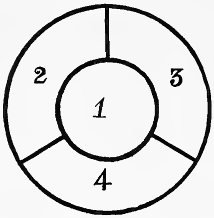
For the construction of these tables, one million six hundred thousand figures were written, and of these the three volumes placed on the table contain about twelve hundred thousand. If the ancient division of the quadrant had been used, the labour would have been more than doubled.

8. The Discharge of Electricity through Olive Oil. By A. Macfarlane, D.Sc., and P. M. Playfair, M.A.

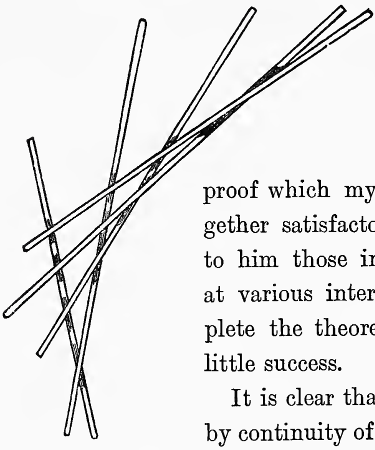
9. Note on the Colouring of Maps. By Frederick Guthrie.

From the Proceedings of the Royal Society of Edinburgh, No. 106, p. 501, it appears the colouring of maps is receiving attention. This note bears chiefly upon the history of the matter.

Some thirty years ago, when I was attending Professor De Morgan's class, my brother, Francis Guthrie, who had recently ceased to attend them (and who is now professor of mathematics at the South African University, Cape Town), showed me the fact that the greatest necessary number of colours to be used in colouring a map so as to avoid identity of colour in lineally contiguous districts is four. I should not be justified, after this lapse of time, in trying to give his proof, but the critical diagram was as in the margin.



With my brother's permission I submitted the theorem to Professor De Morgan, who expressed himself very pleased with it; accepted it as new; and, as I am informed by those who subsequently attended his classes, was in the habit of acknowledging whence he had got his information.



If I remember rightly, the proof which my brother gave did not seem altogether satisfactory to himself; but I must refer to him those interested in the subject. I have at various intervals urged my brother to complete the theorem in three dimensions, but with little success.

It is clear that, at all events when unrestricted by continuity of curvature, the maximum number of solids having superficial contact each with all is infinite. Thus, to take only one case, n straight rods, one edge of whose projections forms the tangent to successive points of a curve of one curvature, may so overlap one another that, when pressed and flattened at their points of contact, they give $n-1$ surfaces of contact.

How far the number is restricted when only one kind of superficial curvature is permitted must be left to be considered by those more apt than myself to think in three dimensions and knots.

10. Remarks on the previous Communication. By Prof. Tait.

(*Abstract.*)

In a paper read to the Society on 15th March last (*ante*, p. 501), I gave a series of proofs of the theorem that four colours suffice for a map. All of these were long, and I felt that, while more than sufficient to prove the truth of the theorem, they gave little insight into its real nature and bearings. A somewhat similar remark may, I think, be made about Mr Kempe's proof.

But a remark incidentally made in the abstract of my former paper has led me to a totally different mode of attacking the question, which puts its nature in a clearer light. I have therefore withdrawn my former paper, as in great part superseded by the present one.

The remark referred to is to the effect that, if an even number of points be joined, so that three (and only three) lines meet in each, these lines may be coloured with *three* colours only, so that no two conterminous lines shall have the same colour. (When an odd number of the points forms a group, connected by *one* line only with the rest, the theorem is not true.)

This follows immediately from the main theorem when it is applied to a map in which the boundaries meet in threes (and the excepted case cannot then present itself). For we have only to colour such a map with the colours A, B, C, D. Then if the common boundaries of A and B, as also of C and D, be coloured α; those of A and C, and of B and D, β; and those of A and D, and of B and C, γ, it is clear that the three boundaries which meet in any one point will have the three colours α, β, γ.

The proof of the elementary theorem is given easily by induction; and then the proof that four colours suffice for a map follows almost immediately from the theorem, by an inversion of the demonstration just given.

We escape the excepted case by taking the points as the summits of a polyhedron, all of which are trihedral; and when the figure is a pentagonal dodecahedron the theorem leads to Hamilton's *Icosian Game.*

11. Note on the Wire Telephone as a Transmitter. By James Blyth, M.A.

It was shown some time ago by Dr Ferguson, and more recently by Professor Chrystal and Mr Preece, that a fine wire attached to a mechanical telephone can act very well as a receiver in a telephonic circuit, provided a make and break, or some form of microphone transmitter, be employed. None of these experimenters, however, have said anything about the action of such a wire as a transmitter. Being struck by the convertibility, in general, of all forms of telephone receivers into transmitters, and *vice versa*, it occurred to me to try how far this wire telephone, as it has been called, could be made to act as a transmitter to an ordinary Bell telephone as receiver. I was much interested to find that it could act in that capacity wonderfully well, as thereby a new element of some importance is introduced into the discussion of the real cause or causes of the action of the wire telephone whether as receiver or as transmitter.

In my first experiments a battery of four Bunsen cells was included in a telephone circuit of small resistance. At the sending station, which we shall call A, an arrangement was made whereby different lengths of various kinds and thicknesses of wire could be inserted in the circuit. At first these wires were inserted by being soldered to the copper terminals, in order to keep clear of loose contacts; but it was afterwards found that all error arising from this source could be avoided by simply clamping the wires firmly between two binding screws. This method, from its greater convenience, was therefore afterwards adopted. To the middle of the inserted wire, and at right angles to it, was attached a fine iron wire about 15 inches long, the other end of which was connected to the centre of the parchment disc of a mechanical telephone. When this wire was stretched moderately tight the transmitting arrangement was complete. At the receiving station, which we shall call B, an ordinary double-ear Bell telephone of small resistance was employed.

When a fine iron wire about 9 inches long was inserted in the circuit at A, any musical sound uttered into the mechanical telephone was most distinctly reproduced at B. Speech could also be

so transmitted, and in one case I managed to do this so successfully that a listener was able to write down an unknown sentence spoken into the mechanical telephone.

I found also that certain lengths of the fine iron wire suited certain voices better than others.

Still using the same fine wire at A, I removed one by one the cells from the battery till only one remained, and found that this did not produce any marked effect on the intensity of the sound as heard at B. The last cell was then removed, and the circuit joined up, and even then a loud sound uttered at A could be faintly heard at B. The only, at least obvious, reason for this effect appears to be, that the vibrations of the iron wire across the lines of force due to the earth's magnetism produces a current sufficiently strong and variable to work a telephone.

The battery being again included in the circuit, a horse-shoe magnet with the line of its poles at right angles to the wire was suspended over and quite close to the wire. This caused the sounds uttered at A to be reproduced at B with distinctly greater intensity than when no magnet was present. To make sure of this a continuous note was sounded at A, and the magnet alternately removed and brought up to the wire. When this was done a marked rise and fall in the intensity of the sound heard in the receiving telephone was observed.

It is to be noticed that, in the case of the fine iron wire, the sounds appeared to be transmitted equally well whether the wire attached to the mechanical telephone was joined to the middle of, and at right angles to, the inserted wire, or to its end, and in the same direction as itself. This observation is of importance, as it does away, in great part at least, with the idea that the effect is produced by variation in the resistance of the fine wire caused merely by the bending to and fro at its points of attachment to the circuit wires.

When the fine iron wire was replaced, all other things remaining the same, by a thick iron wire (No. 12), which had been previously rubbed with a magnet, the sound heard at B was very faint, although audible. It came out, however, very distinctly where the iron wire was heated by a flame to a dull red heat.

With a thickish platinum wire inserted at A the sound produced at B was very faint; but on putting in about twelve inches of fine

platinum wire, the result was almost as good as that obtained from the fine iron wire itself. This is rather a puzzling result to explain, as it cannot arise from magnetisation of the metal, as is probably the case, in part a least, with the iron wire.

Fine wires of copper and aluminum transmitted no sound whatever, although they were treated precisely in the same way as the iron and plantinum wires.

A short strip of cobalt inserted in place of the fine wire at A gave a very distinct result, although the sound was not so loud as in the case of the iron wire. This, however, may be due to smallness of the vibrations arising from the stiffness of the strip.

From all these experiments it is obvious that, some how or other, rapid variations of the current strength are produced in the circuit, and the problem is to explain how these variations are produced. Now the current strength can only be varied in two ways—either by varying the electro-motive force or by altering the resistance of the circuit; and no doubt, in this case, both ways come into play to a certain extent. The former comes into play, inasmuch as the electromotive force will be varied by the motion of the fine wire in the magnetic field caused by itself and by the earth's magnetism. The latter will also come into play, inasmuch as change of resistance will arise from at least three distinct sources. These are (1) varying magnetisation in the wire, especially the iron wire, produced by strain; (2) variations in the temperature of the wire caused by the cooling effect of the air as the wire vibrates; (3) alterations of the resistance caused by the varying strain to which a vibrating wire is exposed. It is possible to arrive at something like a numerical estimate of this last cause of alteration for any particular note transmitted; for, knowing the mass of the vibrating wire, its initial tension, its number of vibrations per second, we can find the variations in the strain to which it is subject; and if we can know how varying strain is connected with change of resistance, as may be got from Sir William Thomson's recent paper on that subject, we have all the elements necessary for making the calculation.

It is right that I should mention that, in making the experiments, I had the valuable aid of Professor Chrystal in the twofold way of helping me to make exact observations, and of suggesting various changes of experiment to bring out or eliminate particular effects.

12. Further Note on Graphitoid Boron and the Production of Nitride of Boron. By R. Sydney Marsden, D.Sc. F.R.S.E., &c.

This note is a continuation of the paper on the "Preparation and Properties of Pure Graphitoid and Adamantine Boron," by Dr R. M. Morrison and myself, published in the Transactions of this Society, vol. xxviii. p. 689, and its object is to correct a mistake which we have made in giving the properties of this substance. We say—

1st, It is not oxidised by air at a white heat, even superficially.

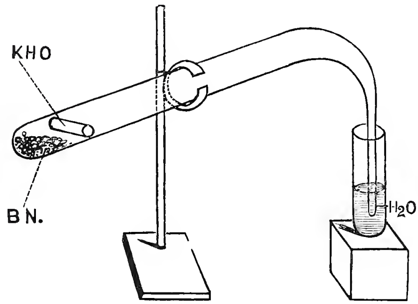
2d, It does not alloy with platinum at a white heat.

I have again prepared a quantity of this substance and examined its properties under Professor Wöhler, and I find that the above statements are wrong. A very fine film of oxide does form on the surface of this substance when heated on platinum foil over the blowpipe, and this film, although very thin and difficult to observe, is sufficient to prevent all further action of the air, and also to prevent its combining with the platinum. This misled us when we previously examined it. If, however, a quantity of it be placed on a piece of platinum foil, and the foil folded over it and pressed down so as to exclude all the air, then on heating it intensely before the blow-pipe the boron at once combines with the platinum and perforates it.

In that paper also we mention a slatish-grey powder which we found surrounding the metal on opening the porcelain crucible. I have made an examination of this powder, and find it consists of a mixture of nitride of boron and amorphous boron, chiefly, however, of nitride of boron, which is formed by the superfluous boron uniting with the nitrogen of the air. If the experiment is prolonged over many hours the whole of the amorphous boron is converted into this nitride, and the powder is then white. The way in which I tested for the nitride was as follows :—A portion of the powder was heated in a hard glass-tube, with solid caustic potash to convert the nitrogen into ammonia, when the following reaction takes place—

$$2BN + 6KHO = 2NH_3 + B_2O_3 + 3K_2O\,.$$

The ammonia is led into a small tube containing water, and tested for in the usual way. In a number of other similar experiments in



which boron was heated with the different metals, this nitride of boron was always found in the crucible surrounding the metal at the end of the experiment.

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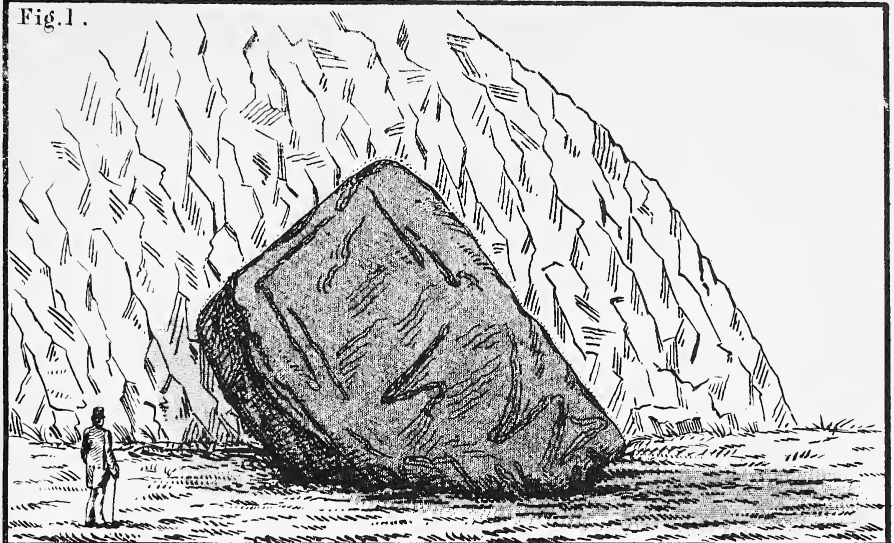
Fig. 1.



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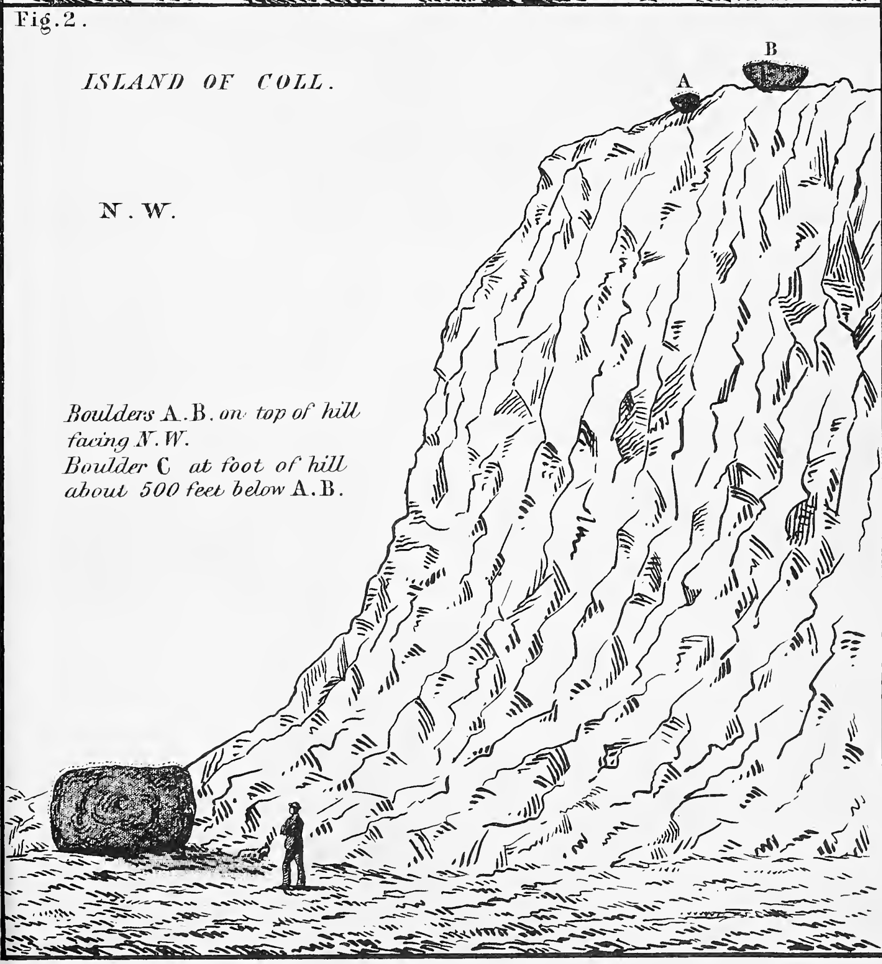
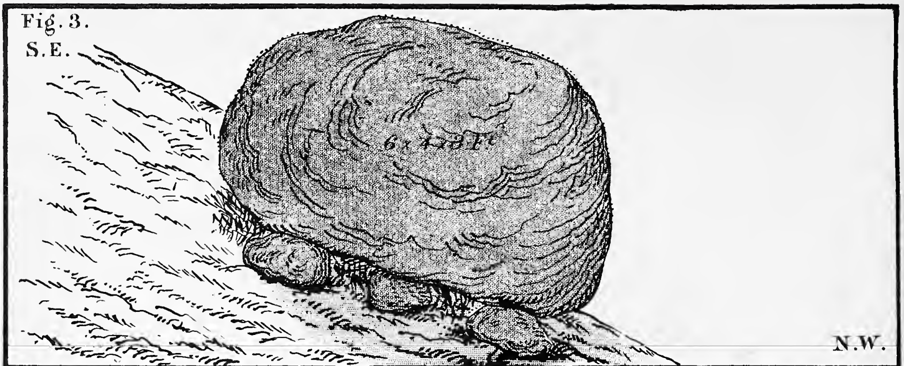

ISLAND OF COLL.
N.W.
A
B
Boulders A.B. on top of hill facing N.W.
Boulder C at foot of hill about 500 feet below A.B.

Fig. 3.



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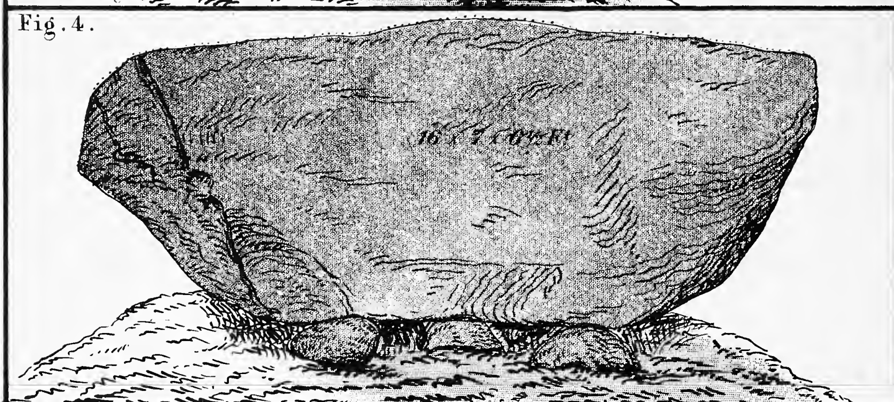
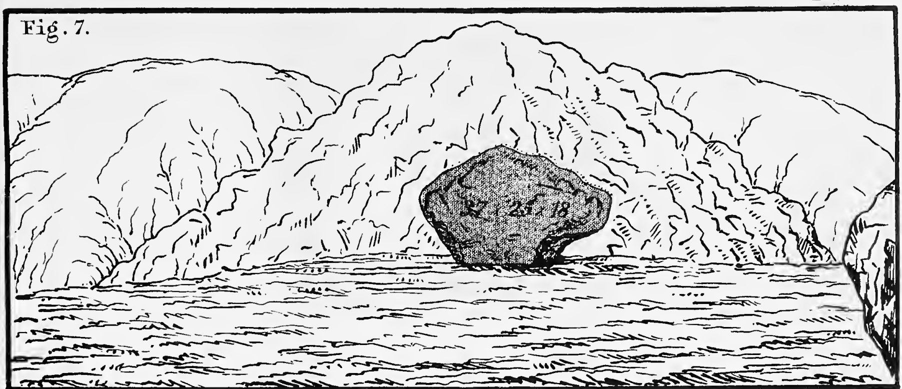


Fig. 5.

Fig. 6.

Fig. 7.



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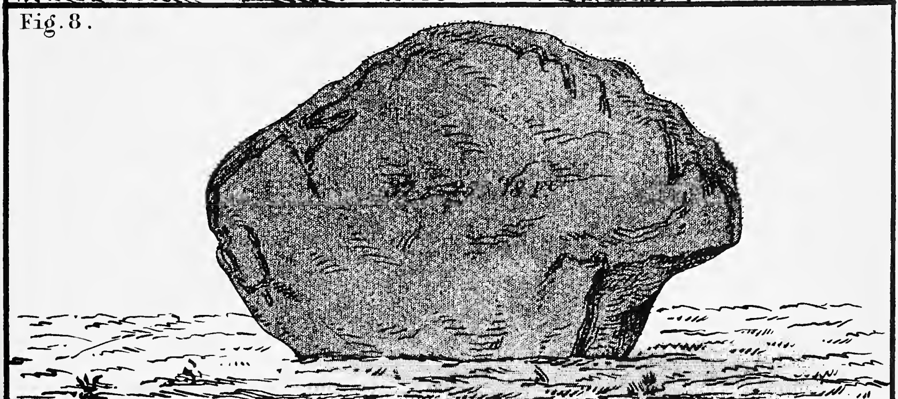

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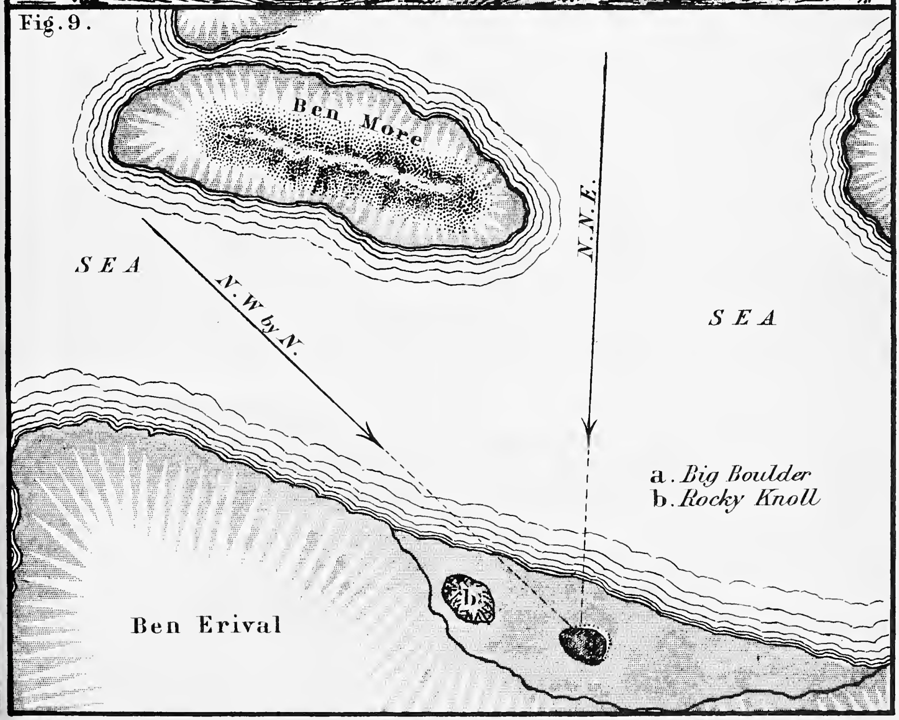

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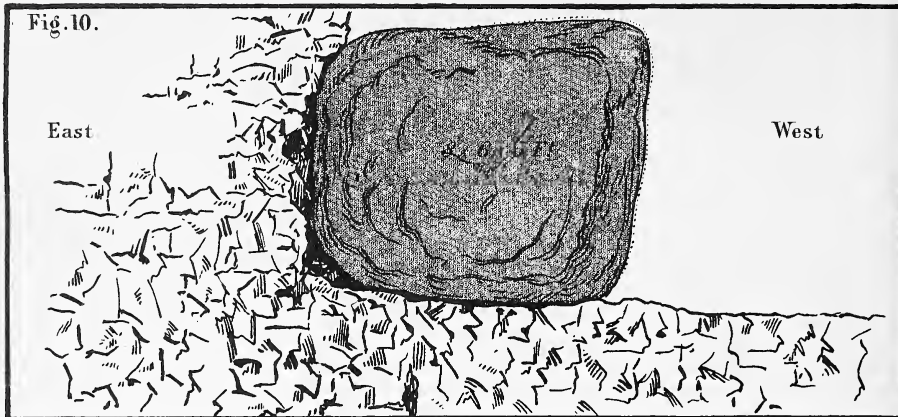

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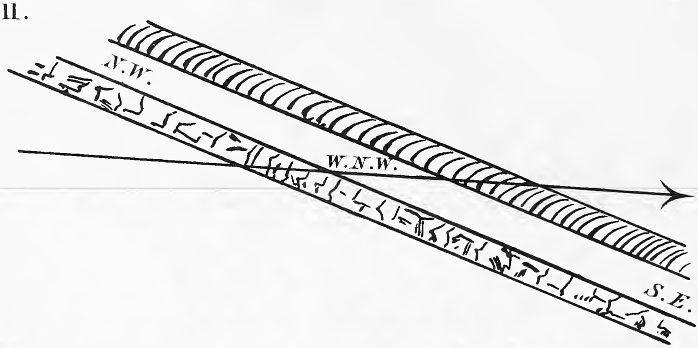

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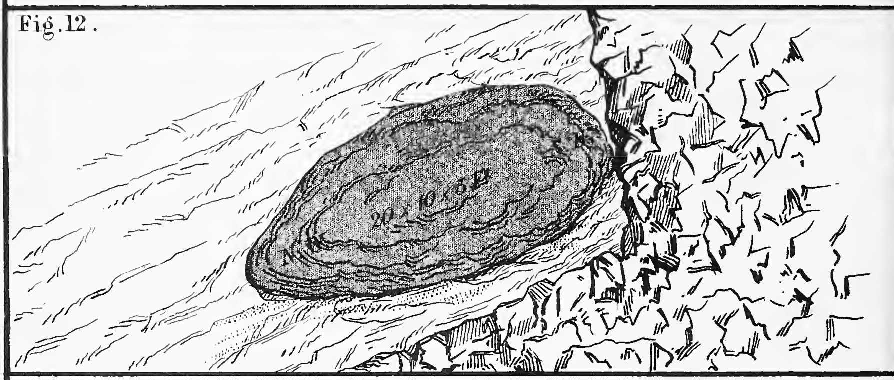

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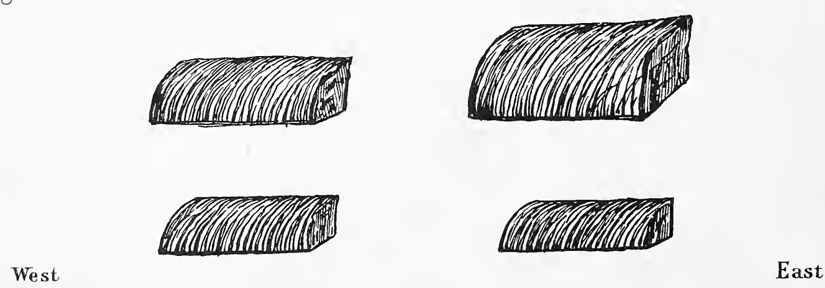

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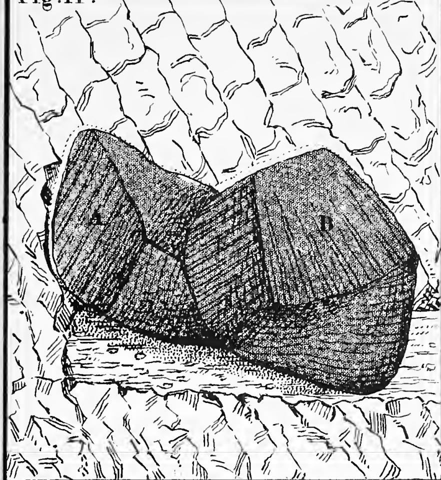

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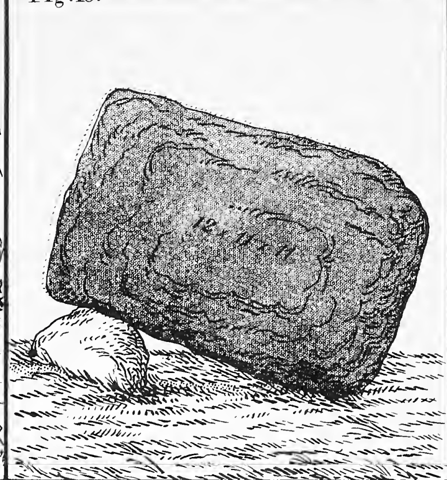

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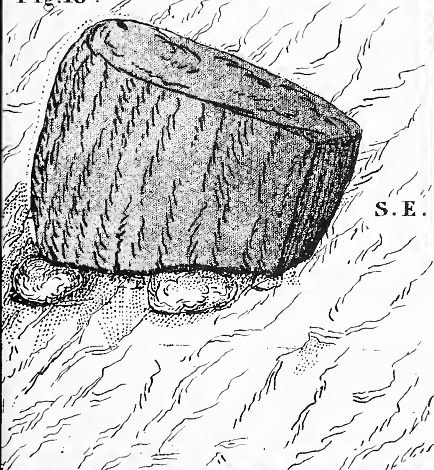

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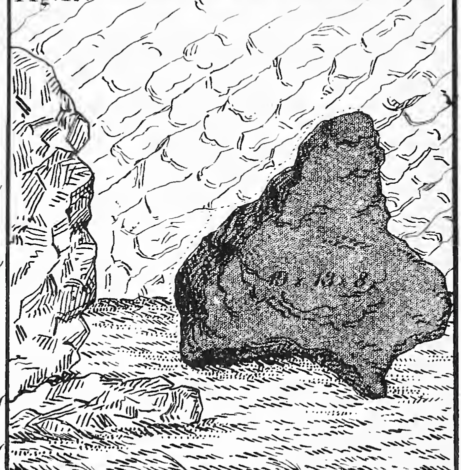

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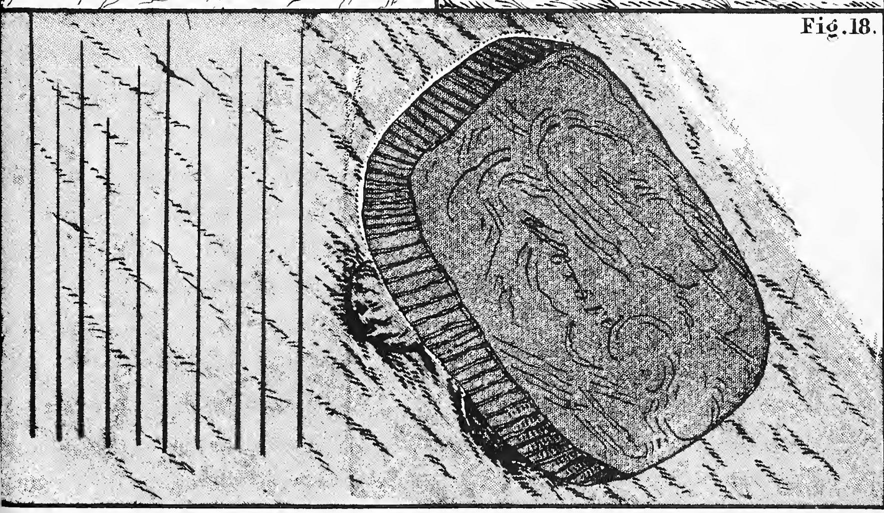

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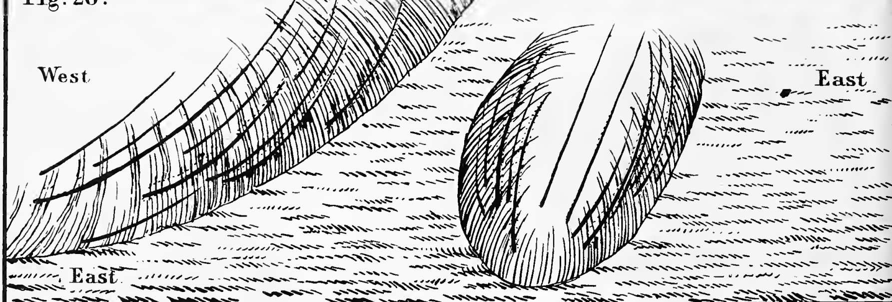
Fig. 20.



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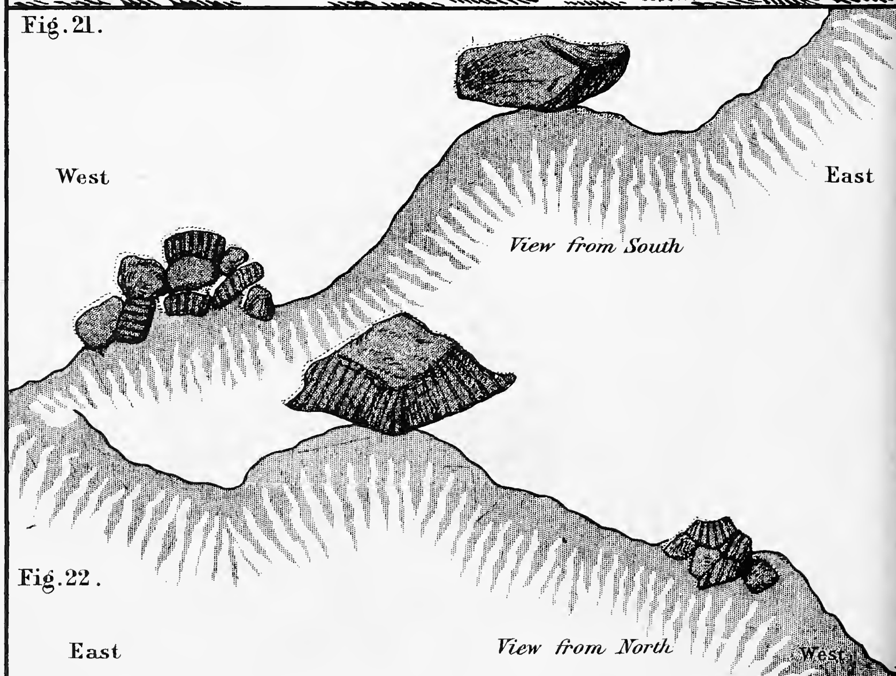

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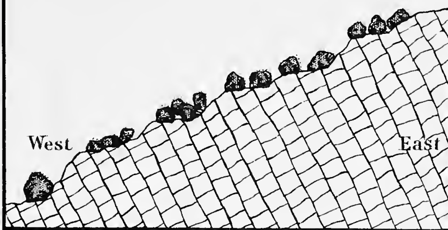

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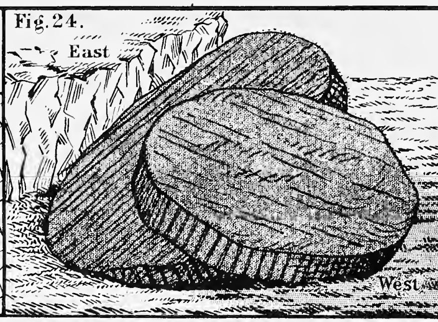


Fig. 25.

Plan of Loch-castle Bay & Valley.

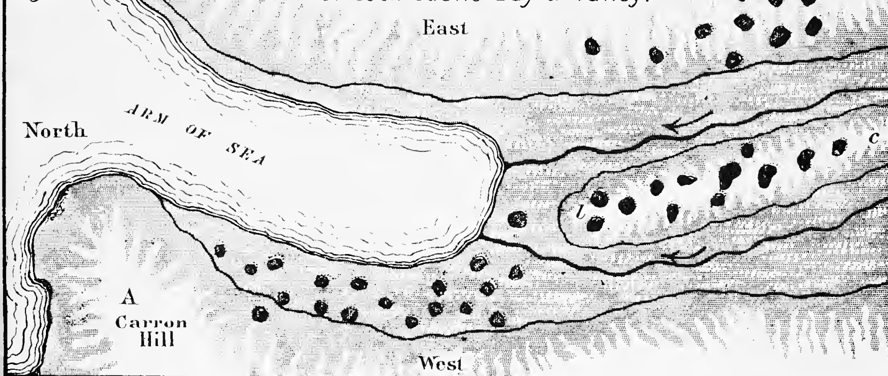
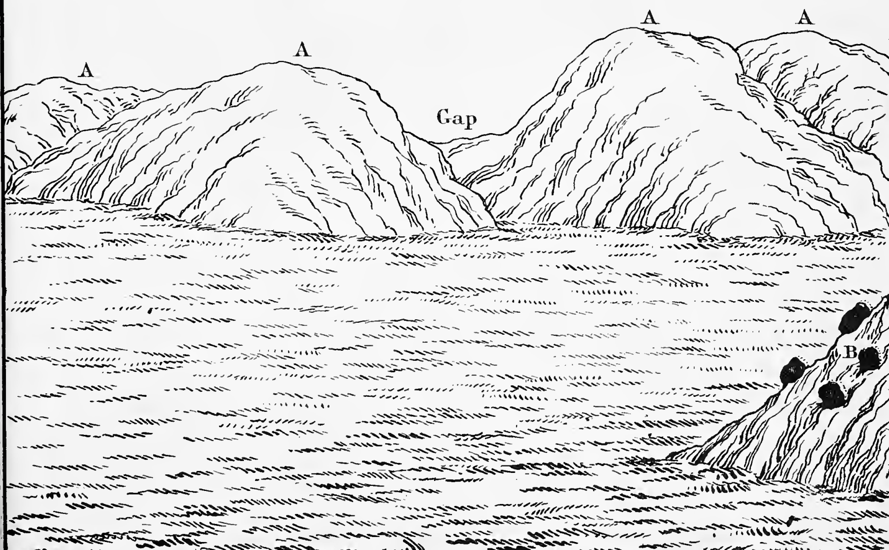


Fig. 26.

Section of Loch-castle Valley.

Fig. 27.



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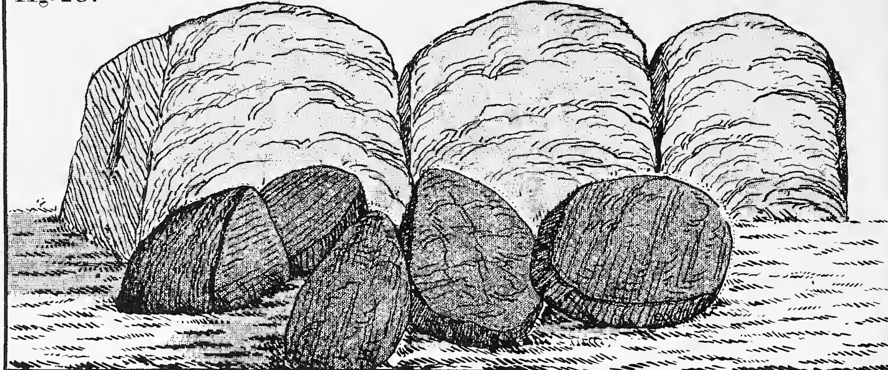

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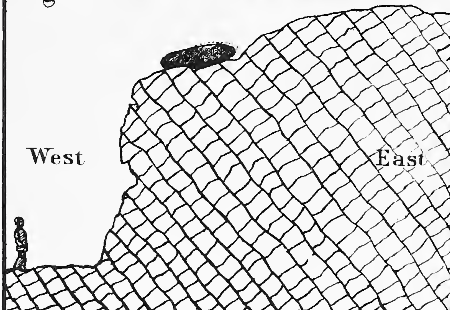

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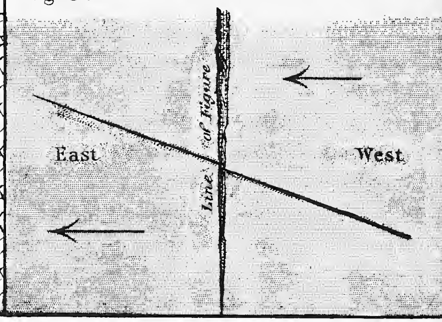

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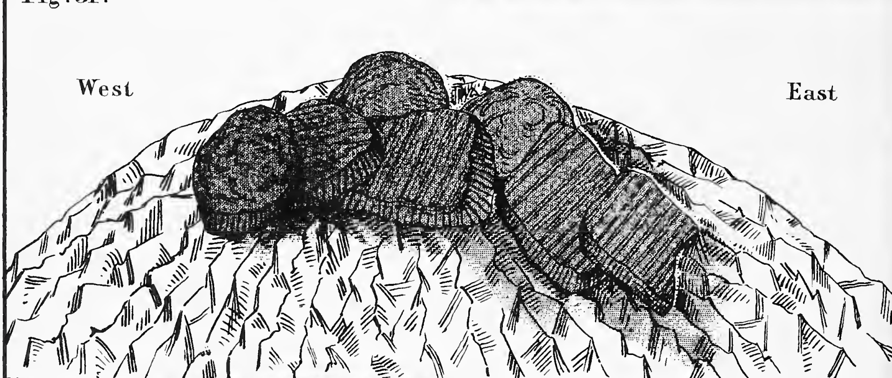

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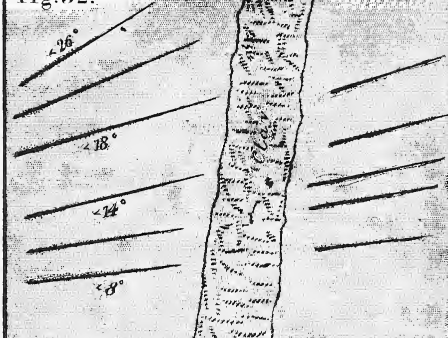

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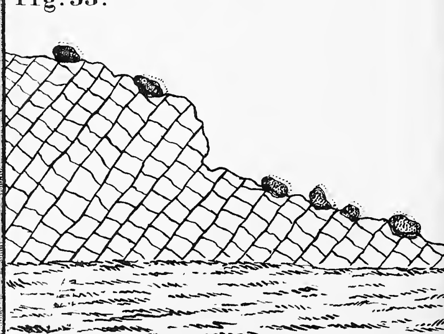


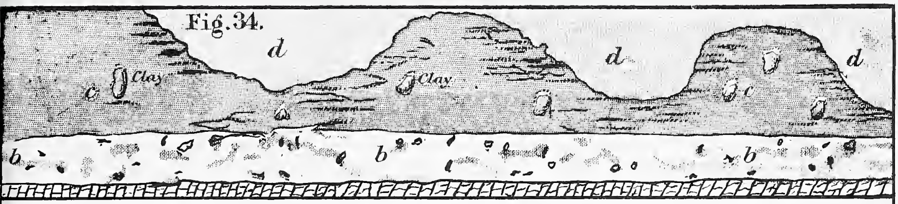


Fig. 34.
d
d
d
c
Clay
Clay
c
b
b
b

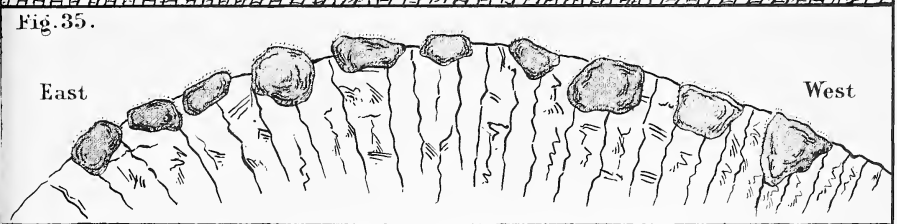


Fig. 35.
East
West

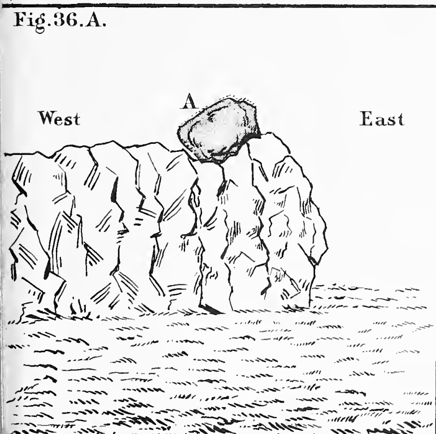


Fig. 36. A.
A
West
East

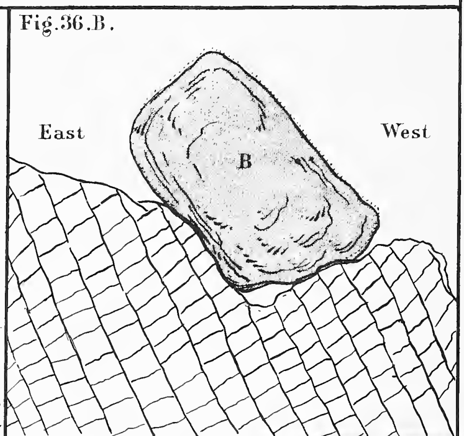


Fig. 36. B.
East
West
B

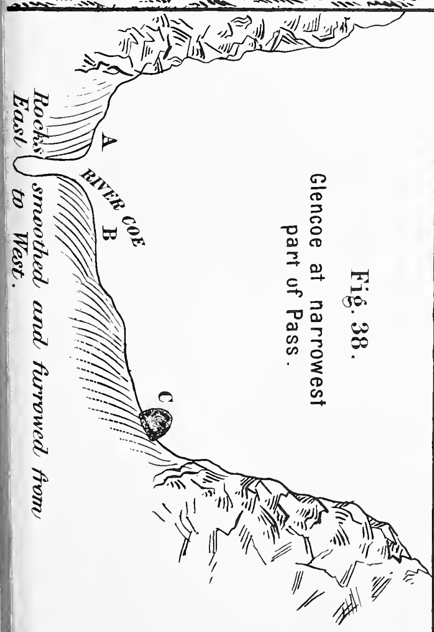


Fig. 38.
Glencoe at narrowest part of Pass.
Rocks smoothed and furrowed from East to West.
RIVER COE
A
B
C

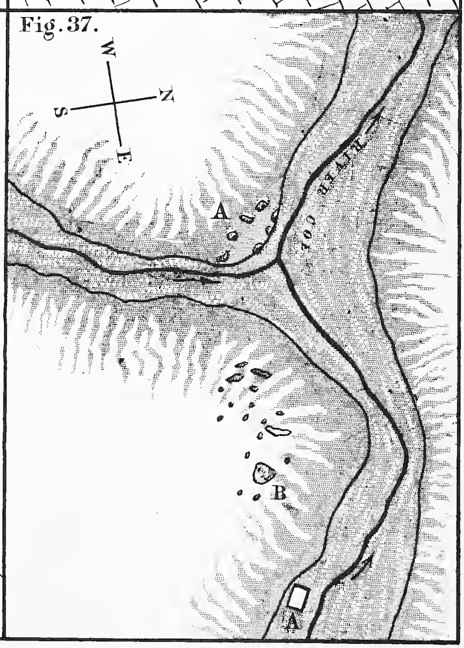
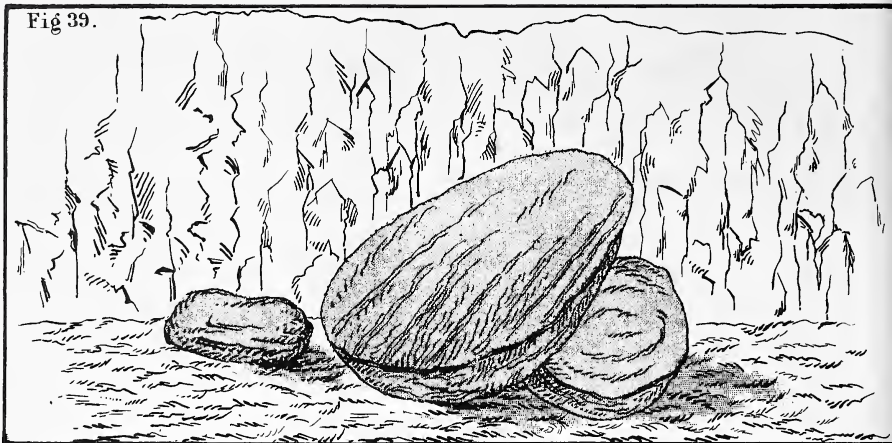


Fig. 37.
W
N
S
E
RIVER COE
A
B
A

Fig 39.



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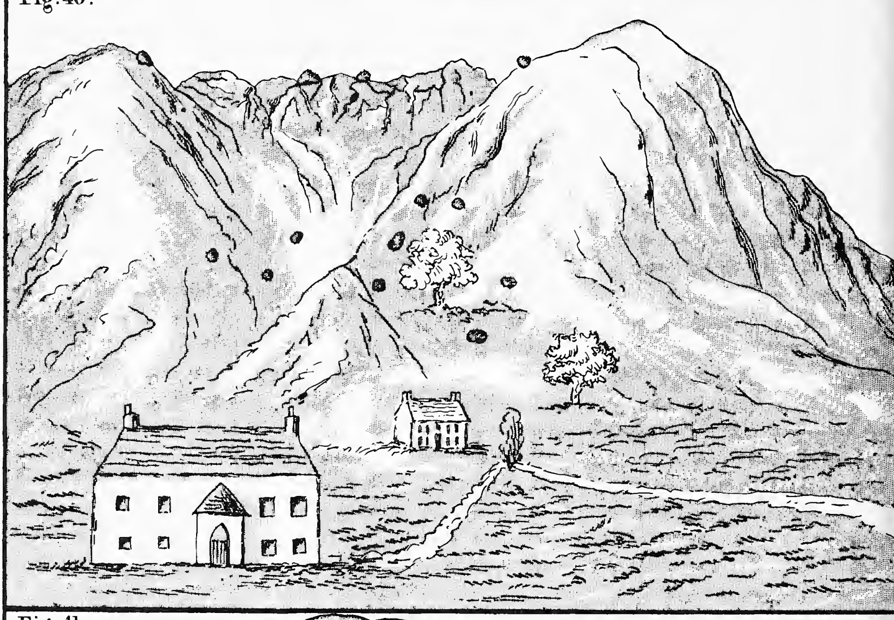

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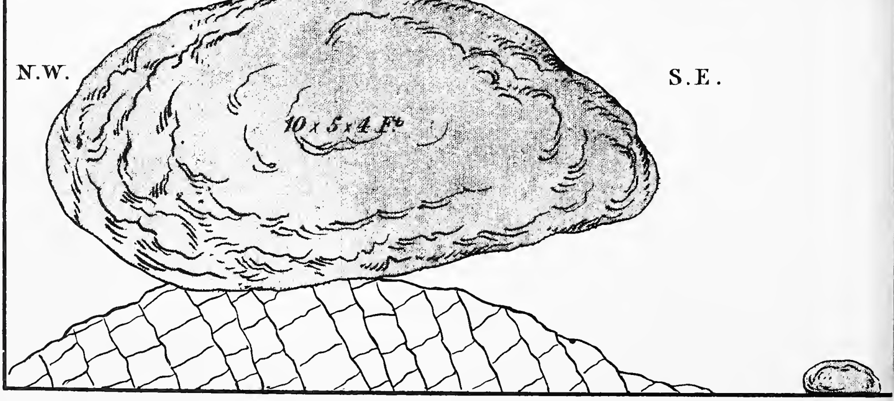

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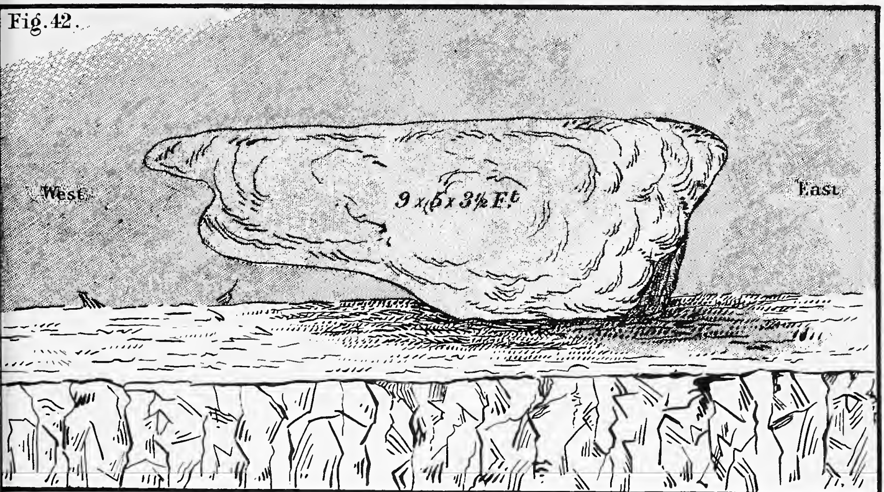

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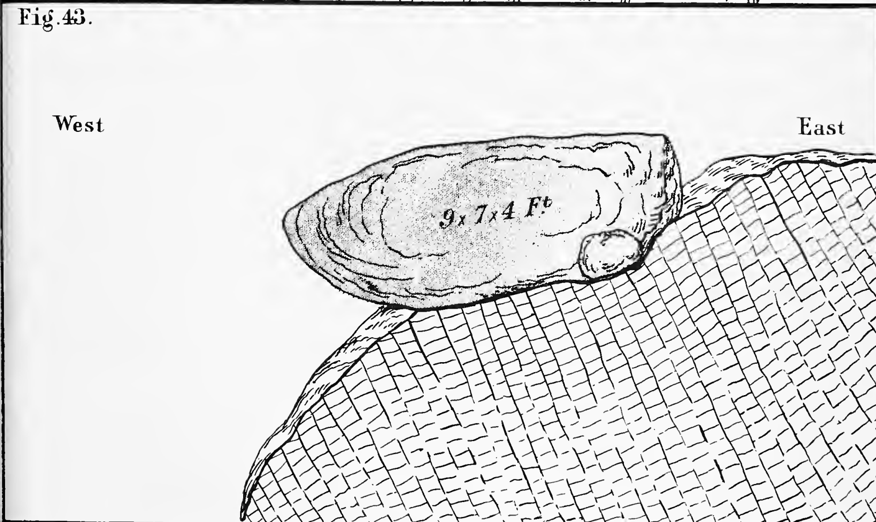

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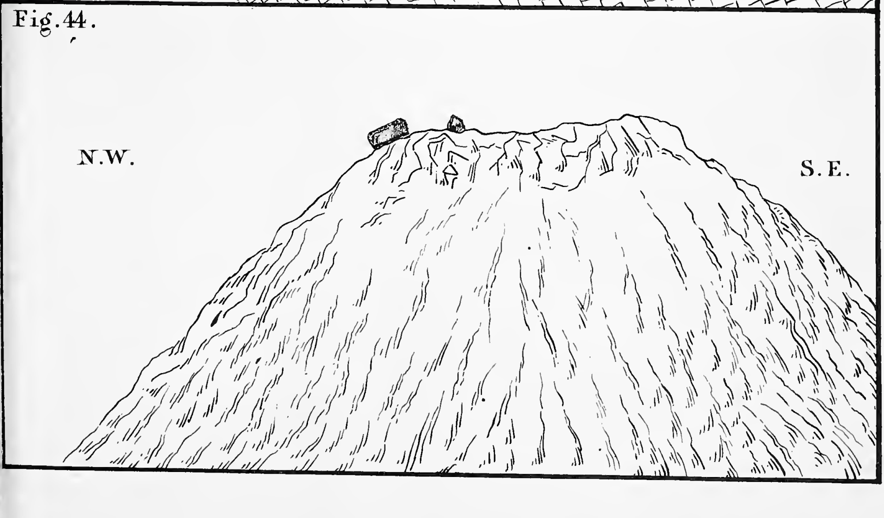

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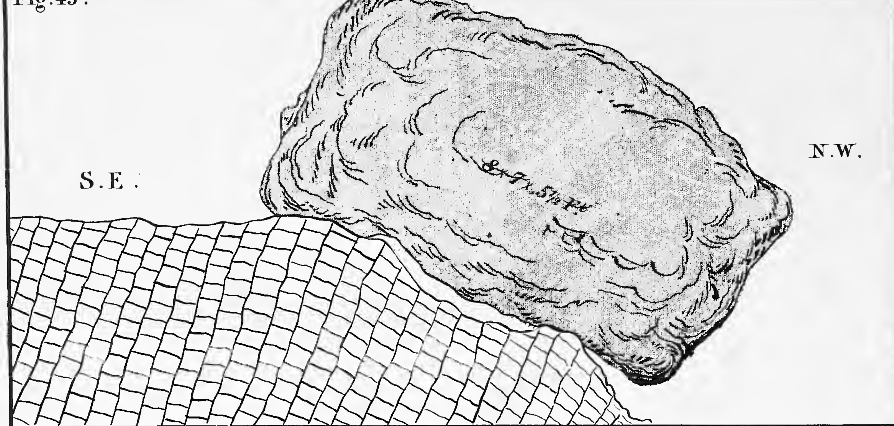

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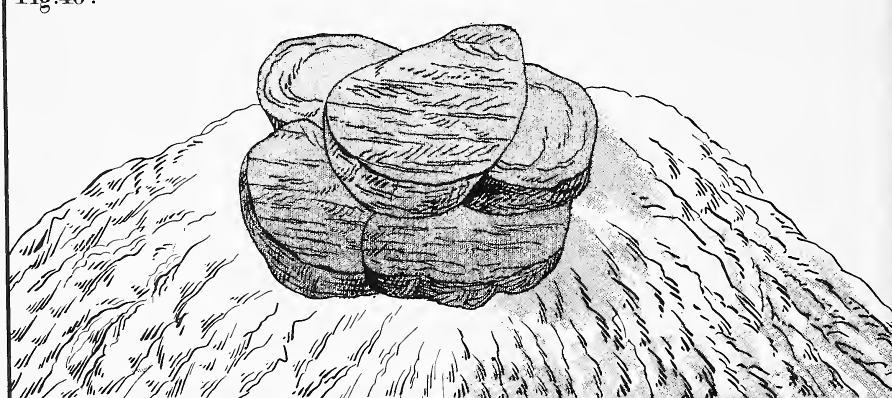

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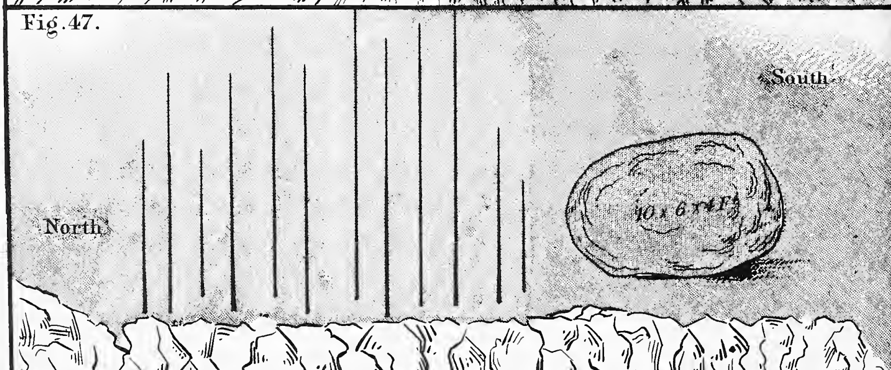

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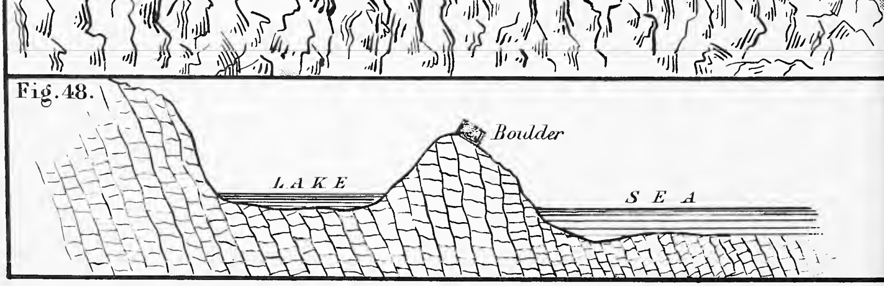

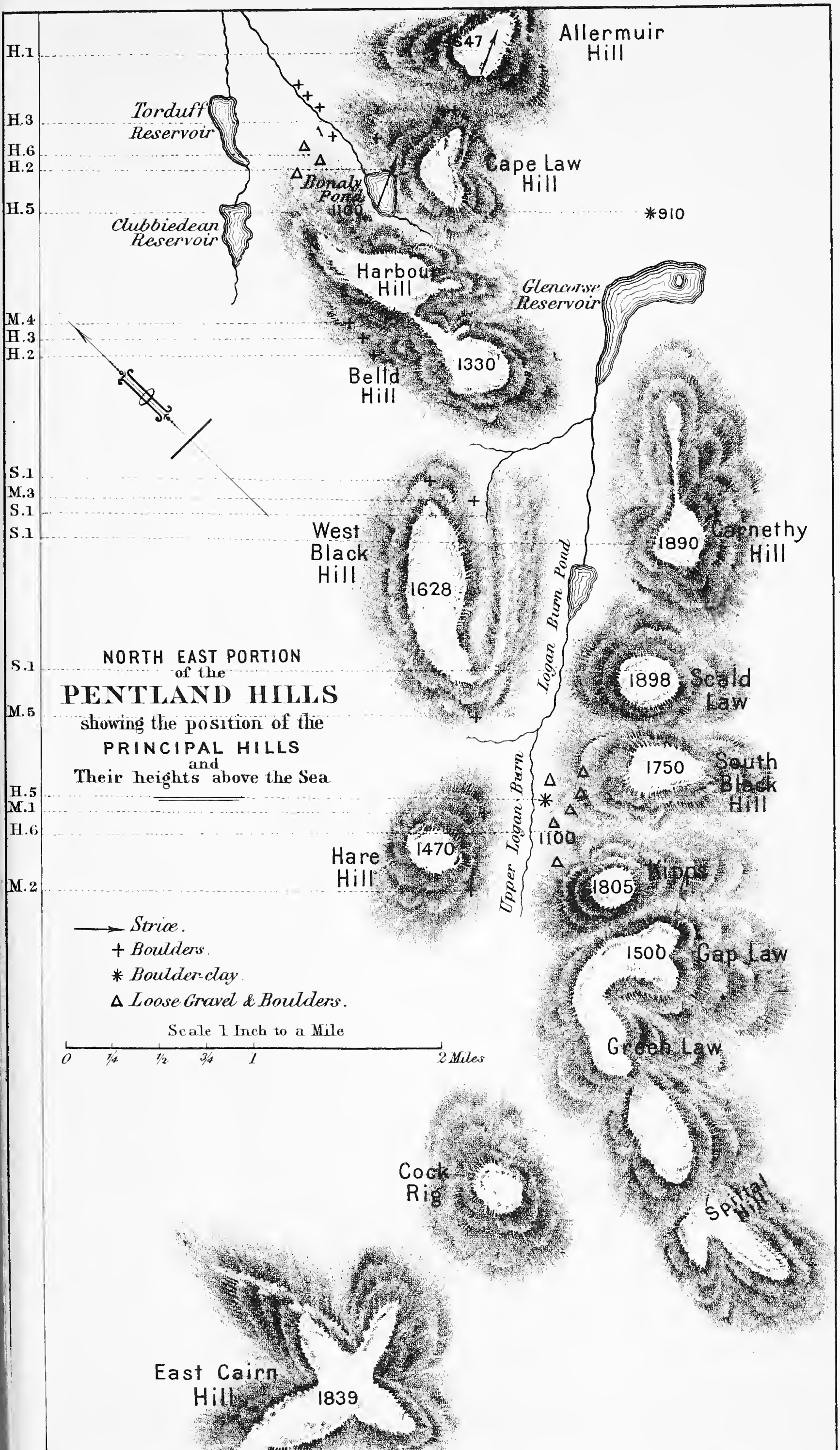

Lithog. by W. & A. K. Johnston Edinburgh

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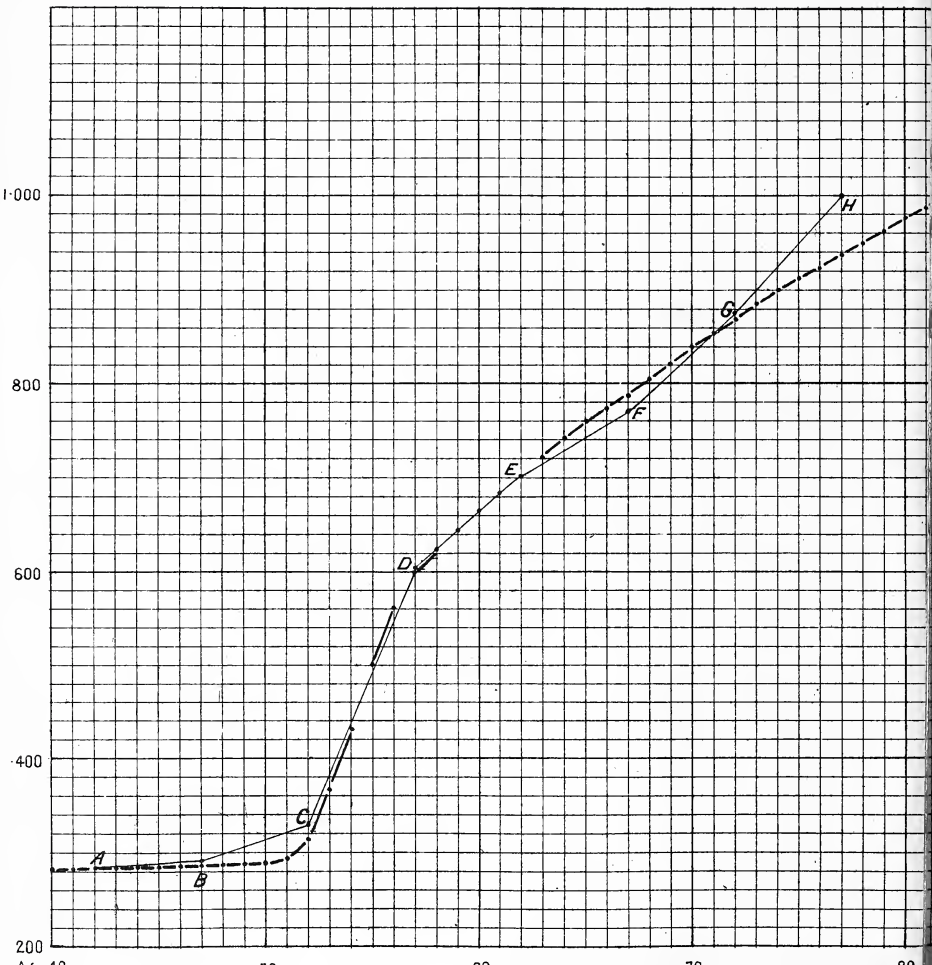

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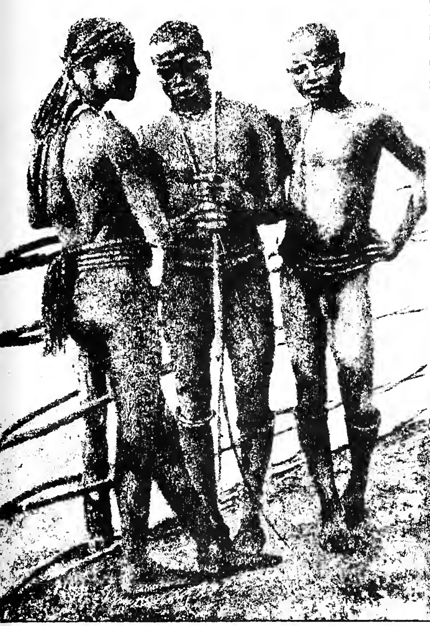


Fig. 4.

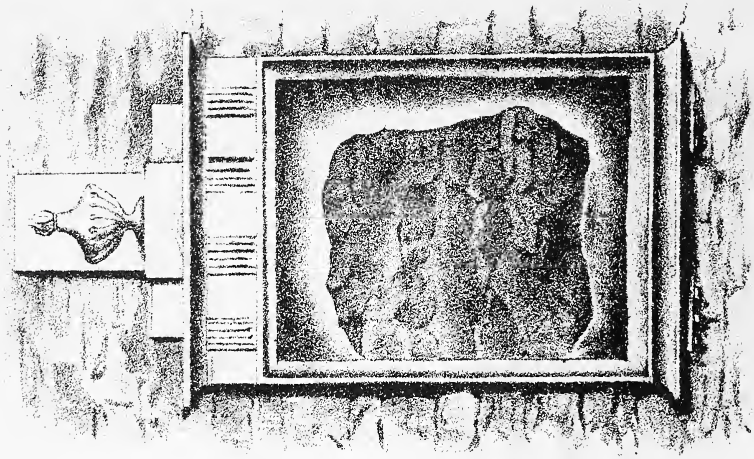
Fig. 2.

Fig. 3.

rom Photos

M^c Farlane & Erskine, Lith^rs Edin^r

To illustrate Prof. Geikie's Paper on Rock-Weathering



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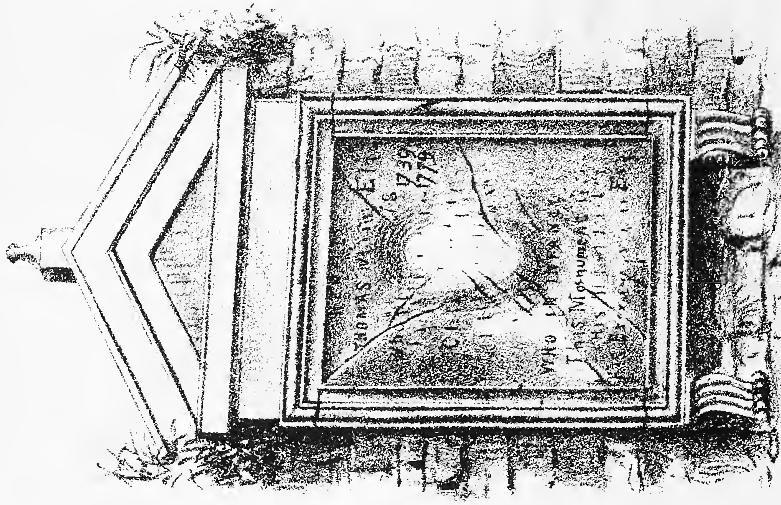

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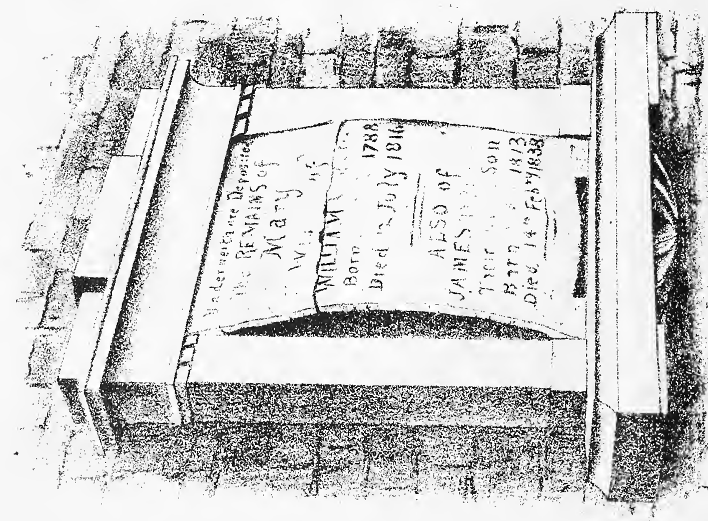

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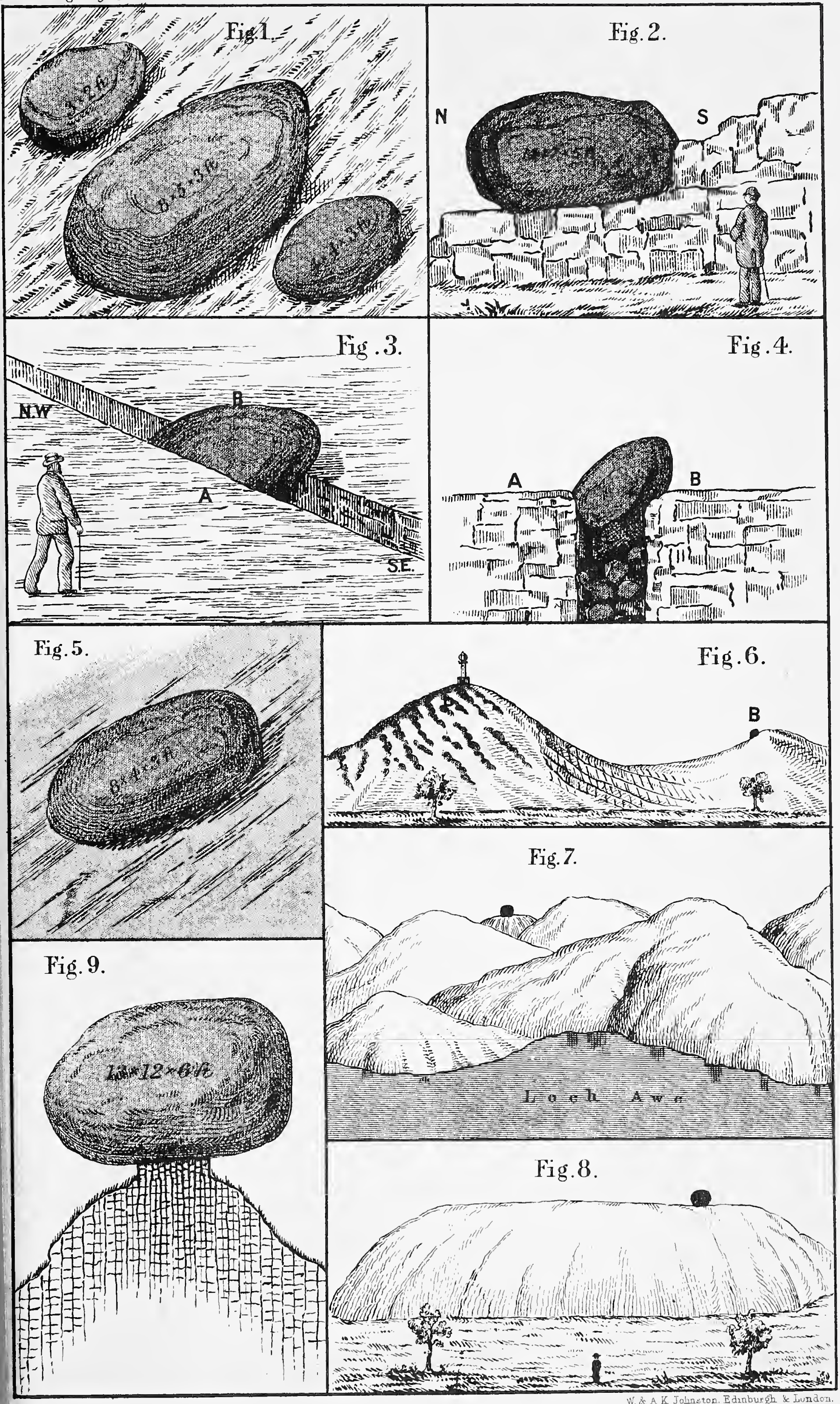
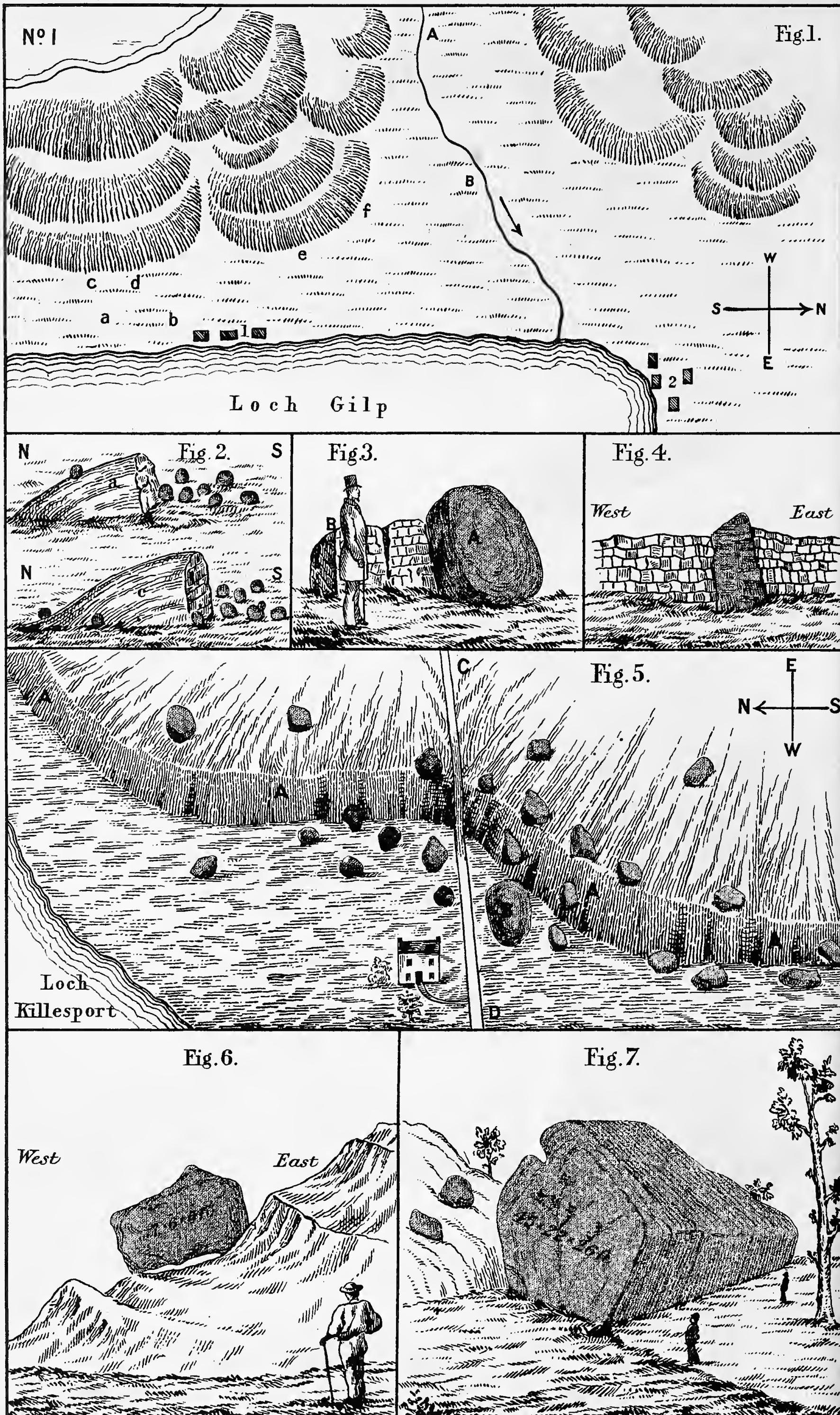


W. & A. K. Johnston, Edinburgh & London.



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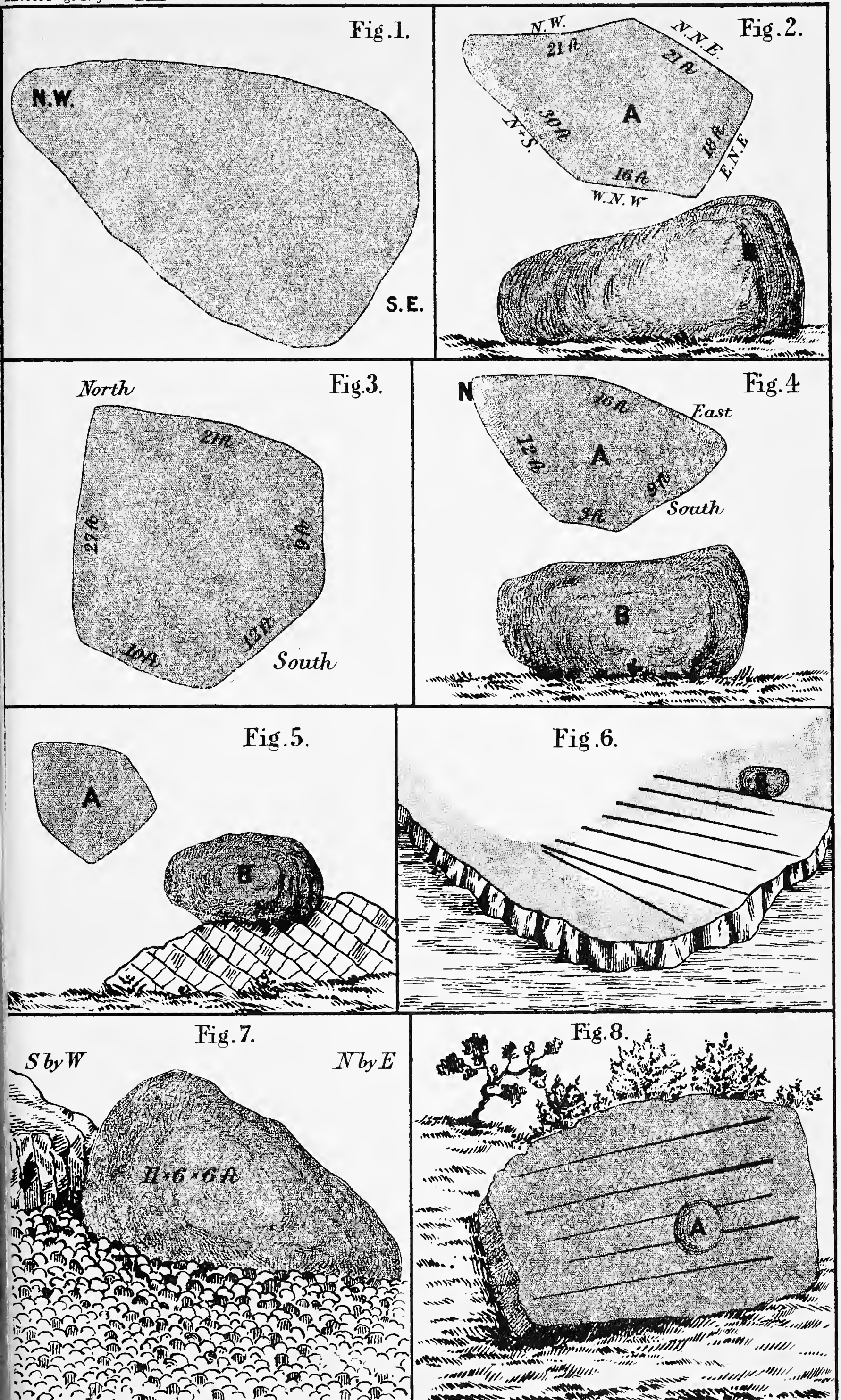

W & A K Johnston, Edinburgh & London.

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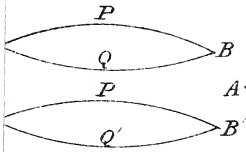

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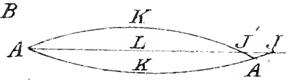

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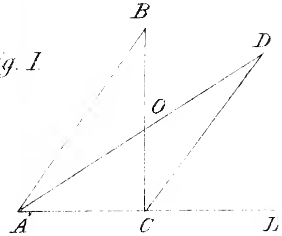

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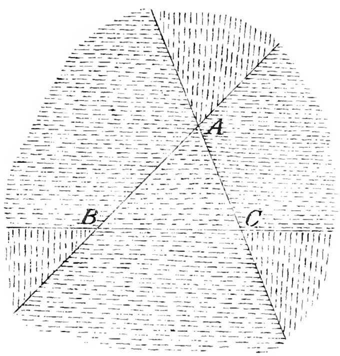

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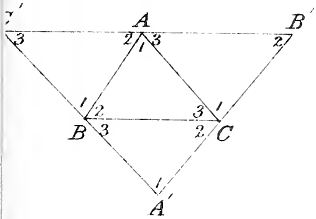

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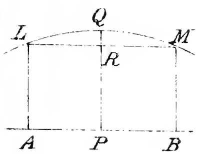

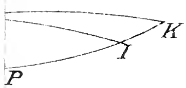

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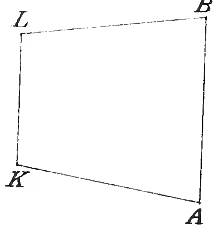

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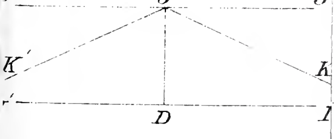

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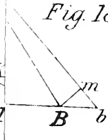
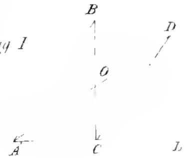


J. Bartholomew, Edin.



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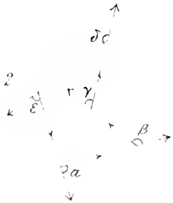

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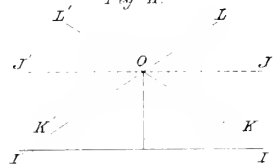

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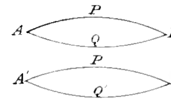


Fig. 12.

Fig 13

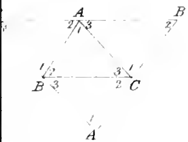
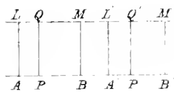


Fig 3



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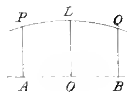

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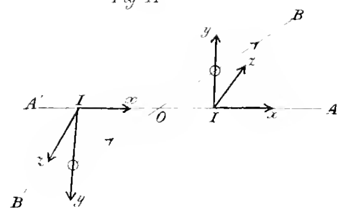

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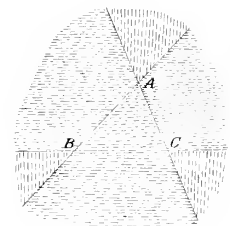

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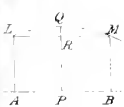

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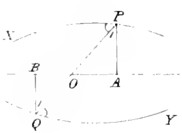

Fig 7

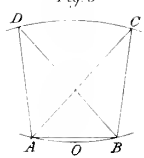

Fig. 8

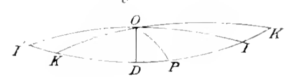

Fig 16.

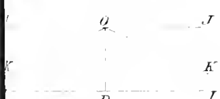

Fig 9

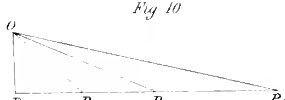

Fig 10

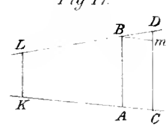

Fig 17.

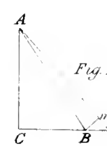

Fig. 18.

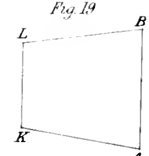

Fig. 19

T.Bartholomew Edin

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